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INSTRUCTIONS

FOR THE

MULTIPLICATION OF WORKS OF ART

IN METAL,

BY VOLTAIC ELECTRICITY.

WITH AN INTRODUCTORY CHAPTER

ON ELECTRO-CHEMICAL DECOMPOSITIONS BY FEEBLE CURRENTS.

BY THOMAS SPENCER.

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P R E F A C E.

IN laying this little work before the public, it may be necessary to allude briefly to the circumstances which led to the publication of a portion of its contents a year ago, and to say something respecting this novel application of voltaic electricity.

A very few years since, I obtained a casual perusal of Sir H. Davy's Bakerian Lectures for 1806 and 1826. From that hour I became strongly impressed with the opinions of that celebrated philosopher, respecting the identity of chemical and electrical forces. Whether electricity was the cause, or only the effect of chemical action, and whether these phenomena were absolutely inseparable, were left by him undetermined. At that period, I was unacquainted with the labours of Faraday, some of them not being in existence, and those that were, being locked up in the ponderous volumes of the Philosophical Transactions, to me inaccessible. I was, however, strongly biassed in favour of that opinion, which recognised in electricity the cause of chemical action.

In the first of these lectures, this great man also pointed out the high probability of the application of electricity to the *analysis of animal and vegetable substances*; and also the still higher probability that mineral formations have been materially influenced, or even occasioned by the agency of electricity; and that the electrical power of transference might thus be applied to the explanation of the principal points in geology. Believing that the realization of this latter opinion would afford a wide and comparatively untrodden field of scientific research, I determined, if my daily avocations permitted, to institute a series of experimental inquiries, that should, if possible, lead to the establishment of true opinions on these important points. It was while following out this resolve, and carefully examining every result, whether it had a tendency to the main object of inquiry or not, that I was fortunately led to a discovery that now promises to be of much use to our manufactures and the arts generally.

When the term *discovery* is used, it is wished to be understood literally, and for the following reason; that I have never seen the fact even hinted at, or in any way pointed out in scientific works, until my own

application of it, nor do *I believe such to be in existence*. I trust this piece of apparent egotism will be excused, when it is mentioned, that my friends have felt more annoyance than myself, at statements made by lecturers on this subject, to the effect, that the facts and their application have been pointed out frequently, and that I have improved a process already known. I may say, however, that *such is not the fact*. On the contrary, I do most confidently expect and hope that the processes here given, will be improved by the practical man in the workshop, inasmuch as they are only given as the best that occurred to me; but it is highly probable that, had I been pursuing this branch of the subject more with a view to pecuniary profit than scientific research, it would have gone from me in a more complete form. On the other hand; my necessarily imperfect labours have received the public approbation of the highest scientific authorities in such a manner that I cannot but feel gratified.

When the first results were obtained in 1838, they were shown to several persons in Liverpool, as triumphant realizations of what we might ultimately expect from a further application of electrical forces, which they were inclined to look on as the fond dreams of an enthusiast. I have reason to believe my character for veracity suffered when I showed a fac-simile of a farthing in copper, it being, as I have since understood, supposed that I had obliterated one side of *a real coin* by an acid, and exhibited the other as my own product. Nay, in Birmingham, on showing some medals so obtained, to an eminent die-sinker, I was kindly advised by him not to risk my reputation by a further exhibition, as they were less likely to be deceived *there*, than in such a town as Liverpool.

When I had brought the discovery up to the point stated in the paper detailing its progress, the facts were announced by letter to the secretary of a scientific body in Liverpool. It was read at a meeting of the society. Specimens of the capabilities of the discovery were shown, and some members present stated their knowledge of my having been engaged on the subject for a considerable period. Having judged it necessary to say this respecting the discovery itself, I shall now add a few words respecting the occasion of its first appearance before the public.

The paper, as since printed, was written with the intention that it should be brought before the Birmingham meeting of the British Association, the secretary of which body was written to some months previously. A satisfactory answer was received, stating that my paper might be read at the mechanical section, by myself, if present. I was perfectly satisfied with this, although I could not help thinking its proper sphere would have been the section of chemistry. However, I made not the slightest objection to this arrangement, but proceeded to Birmingham, taking with me a number of specimens produced by voltaism, with

printing and impressions from copper plates, and the plates themselves produced by it, with two forms of the apparatus, all of which I intended should illustrate the paper.

Through mismanagement of some individuals connected with that section, I was not permitted to read it. The particulars of the transaction have been already before the public, and it is unnecessary to detail them here, nor should they have been mentioned were it not to show the reason why the matter was not laid before the public at an earlier date. It is true the paper might not have been suited to the occupations of that particular section, but the officers connected with it were not aware of its nature, nor was this made any ground of objection.

On my return to Liverpool, the paper was read at the next meeting of the Polytechnic Society, and the different methods of procedure explained, both of which were printed by the society, and the whole edition distributed gratuitously to the public.

Applications have been made to me repeatedly, to know whether I had been successful in any attempt to precipitate other metals besides copper.

As might be expected, from the numerous experiments made, and the period of time I have been engaged in them, I have almost necessarily attempted to apply the electro-chemical process to nearly all the solvent metals, including gold and silver. The results have not yet been made public, nor the particular modifications of the process, for the simple reason, that nothing has been done which I conceive might be applied usefully or profitably. I may add, that if, in the first instance, I had been merely able to precipitate copper on copper in the solid state, and nothing more, I should never have thought it worth while to call public attention to the subject. It is, however, true, that at an early period of these experiments, I was enabled to deposit gold, silver, and platinum, but to a limited extent. These have been made public as to the facts, but not as to the detail.

As respects platinum, it may be deposited from its chloride, by the same method as copper, but it is in the form of a brittle black powder, at present affording small hope of usefulness. Gold may also be precipitated from its chloride, on copper or silver, by the apparatus for copper, described in the body of the work. Since my processes have been published, M. D. La Rive has adopted an inefficient method of gilding by the same process, by putting the metal to be gilt in a bladder with the solution of gold, and having a piece of zinc attached in another solution.

The proper methods are precisely those which apply to copper, with these exceptions:—porous earthenware answers best as the separating medium, or should the operation be performed in the secondary cell, the battery should be excited with cold water only, and the electrode opposite the one to be gilt, should be of gold. Next, the solution of gold should be very weak, almost colourless, and a few crystals of *car-*

bonate of soda should be placed in the bottom of the vessel containing the gold. The solution should be pure and free from copper. As yet, however, this process does not augur well, it requires being repeated once or twice, and when attained, the coating is *excessively thin*, and may be removed by the friction of the finger; but it becomes rather more tenacious when burnished.

It might naturally be supposed that, by repeating the operation, the deposit would acquire thickness. The means, however, by which this may be effected, are as yet unknown. The desideratum being to deposit *gold on gold* in the solid state. Whenever the silver or copper becomes *completely* covered with a thin film of gold, *it ceases* to be silver or copper, as respects the surrounding solution, or its electrical properties; and no more than this one coating can be deposited in *the solid state*, by any electrical process, *as yet* discovered. It will, under these circumstances, at once be obvious, that the coating must be *chemically* thin. Were we able by this process to deposit a second coat, there would be nothing to prevent a third, and so on; in that case flowers of gold, or other ornaments in relief, might be deposited on silver, or *vice versa*. I have been, however, more successful with silver, under certain modifications.

I do not think that the voltaic method of gilding will supersede that already in use, unless the process be still farther improved; because, by the present process, we cannot obtain a coating of sufficient thickness for practical purposes. By the common method, articles may be double or treble gilt, and even with single gilding may be handled with impunity. It must also be taken into consideration that metals may be gilt by *merely immersing them in* a solution of gold, with *bi-carbonate of potass*, *without the application of galvanism*.

Since I first made this process public, I have from time to time observed in the public prints accounts of the deposition of other metals in the solid state, many of which could never have been made, but must have only existed in the imagination of their authors. That nearly all the metals will ultimately be brought under subjection to VOLTAIC ELECTRICITY, and that too for useful purposes, I cannot hesitate to acknowledge. The precise methods of procedure are not yet before the public; but that numerous other useful facts and applications will necessarily grow out of those that have been ascertained, those who are best acquainted with the laws and universality of this mighty power cannot doubt.

When we take into view the fact that every form of matter we are acquainted with is saturated in definite proportions with this wonderful power, lying, as it were, dormant, but never under any circumstances refusing to be drawn forth to perform its functions, when the proper means are applied; and when we recollect that it can never be dissipated,—the force, it is true, may be misapplied by ignorance, but not

wasted—that it fulfils its assigned office during its passage from one body to another, from whence it may be again and again recalled with definite, yet undiminished vigour. When we also take into consideration that scientific men, in every quarter of the globe, are at this moment running a race with each other, vieing who shall be the first to make another useful application of this principle; and that the battery is rapidly becoming a tool in the workshop, and an instrument in the surgery;—with these facts before us, surely little doubt can be entertained that we are on the threshold of still greater improvement.

It might be expected that this work should have been illustrated by the process it describes. Under other circumstances it would have been so: electricity is now capable of performing tasks of greater difficulty; but, in the present instance, it would have rested with myself to have done it, the manipulation of the process not being sufficiently known to the engraver, although rapidly becoming so, and my own spare time has been occupied with subjects which I have deemed of more importance than the drudgery of repeating experiments, with the results of which I was already acquainted. Within the last year I have sent specimens to the public, exhibiting its capabilities in every ordinary department of art; and a repetition of this from me is surely uncalled for, my object being fully attained when the best and most expeditious method has been pointed out.

Latterly, I have seen voltaic copies of large engraved copper plates, exhibiting the most minute and microscopic lines, with all the fidelity of the originals; even the burnished appearance of the surface, which, under other circumstances, they never could have acquired, but from the peculiar friction given to them by the hand of the workman. These may be multiplied *ad infinitum*.

There are few branches of manufactures in any way connected with art, that will not benefit by the adoption of this principle. Already it is in active operation among calico printers. Potters can now afford to pay for the highest art to design and engrave one plate, as they may have any number of duplicates equal to the original, at little more expense than the price of the copper. The copper matrix for type founding, however elaborate, may now be produced with the greatest facility, and at a trifling expense.

Wood engravings may now be copied in copper to any extent, and the finer lines which could not be obtained in the originals, may be put into the copper fac-similies with facility. Even in stereotyping, it may ultimately be found as economical as the method in common use, and certainly much better.

Elegant designs are deposited on plates in relief, from which embossed cards are printed; in short, there is scarcely a department connected with the elegancies and refinements of life, where I do not hear of its application; but since I discovered an efficient method of

metallizing the surface of non-metallic substances, a copper statue may be taken from a plaster of paris mould, in little more time than would be required to deposit the same thickness on a medal.

Few who watch the progress of events can doubt, that had this discovery not been made at the present period, a very brief additional time would have brought it under public attention. Scientific facts were all tending toward it. The great discoveries of the last age were being condensed and combined into the elements of learning for this. Trains of scientific thought which had been long and curiously laid, only required the aid of the match to explode them simultaneously.

In this little work I have laid the experience I possess on this branch of scientific art before the public, but doubt not that a few years in the hands of the many will improve it. It is true I have been a long time occupied with the subject, but it must be taken into consideration, that my means of cultivating science are limited, the hours it occupies being stolen from those usually set apart for relaxation and rest, my ordinary avocations being of a laborious nature; yet I have felt so much real gratification in the pursuit, so much exquisite pleasure at the results of successful experiment, that I consider my own reward as ample.

The forms of apparatus for producing these results, have been termed by one portion of the public, "electrotypes;" by others, their name included a portion of my own, neither of which I think correct. I would propose that the instruments themselves should be termed "ELECTROGRAPHS," and that the whole art of applying electricity to the production of such works of art, should be termed "ELECTROGRAPHY."

LIVERPOOL, *August*, 1840.

INTRODUCTION.

ON ELECTRO-CHEMICAL DECOMPOSITIONS

BY

FEEBLE CURRENTS.

FEW branches of natural philosophy have received more cultivation, within the last few years, than the science of galvanic* or voltaic electricity.

It might be inferred that a corresponding advancement had taken place in our real knowledge of this subtle agent, or that the laws by which it is governed had been applied generally to the solution of many hitherto unsolved phenomena. Such, however, is not the case. It is true, the observed facts are ably recorded, and widely diffused, but they stand, as yet, isolated, being regarded by the scientific world at large as curious merely, but neither sufficiently numerous or conclusive in themselves to warrant us going further. General inferences have occasionally been drawn, and theories from time to time advanced, but they have been urged with more enthusiasm than philosophy, and have retarded rather than advanced the object their authors had at heart.

The day, however, is not distant, when the numerous facts that are scattered over the pages of our periodical and other scientific literature, will be seized on by some powerful mind,—condensed into one grand whole, and the foundation of new and important scientific views firmly laid, which, in all probability, will do as much to revolutionise modern physics, as the discovery of Copernicus has already done to astronomy. One or two reasons may be assigned why the progress of this science has not been commensurate with its cultivation; one is, that more attention has been given to the supposed implements of research, than to the researches themselves, more persons having applied their energies to the construction of new and improved forms of the battery, than to establish the laws which govern the science itself. To this, however, we have a splendid exception in our countryman Faraday, to which we may fairly add Mr. Sturgeon. In France the cultivation of the

* The world is equally indebted to Galvani and Volta,—to the one, for the discovery of that modification of electricity which bears his name; and to the other, for explaining and showing from what substances, and under what circumstances, it might be invariably obtained; and finally, for his invention of the pile, of which the various batteries of the present day are merely modifications.

science has hitherto been chiefly confined to Becquerel. The discovery of electro-magnetism in 1820, by Oersted, has in some degree tended to divert attention to this kindred science, but more especially, since the probability has become manifest, that it may be usefully applied as a motive power. Hence it has become a desideratum to form a battery combining great intensity in small compass, together with economy and equability of action. As might be expected, various expedients have been resorted to, with more or less success; but on the whole, very great improvement in this department of the science has taken place.

The researches of Mr. Faraday have been of a much higher character. They have been principally and successfully aimed at the establishment of the grand fact—*that the forces termed chemical affinity and electricity, are one and the same*. This great principle had been previously surmised by Sir H. Davy in 1806, and again in 1826 by the same philosopher, but in both instances vaguely, and the experimental proofs adduced on these occasions were far from being complete. Mr. Faraday, however, by a series of unique and simple experiments, combined with acute inductive reasoning, has set this important question at rest. It is true, Dr. Wollaston, as early as 1801, attempted to prove by a set of beautiful experiments, that the evolution of both electricities was due to chemical action; but in these, as well as in subsequent investigations, electricity was adduced only as *an effect* of chemical action, not as a *cause*.

Thus the study of voltaic electricity has become *indispensably necessary*, to further the researches of the philosophic chemist, as in it he must henceforth recognise the motive power of *all chemical change*.

The great question of the nature of electricity, is yet involved in doubt, although it has engaged the attention of many of our most eminent philosophers. Whether it is a force in nature, of which the human mind can form no possible conception,—or whether it is a subtle *atmosphere*, by which the normal molecules of bodies, or even the ultimate atoms of matter, are surrounded—is a problem at present involved in profound mystery. I cannot, however, help holding that opinion to be best supported, which considers electricity to be a real fluid, or at least *having materiality*; and, indeed, it is difficult, if not impossible, for a chemist believing in the atomic theory of definite proportions, to conceive it has *not*. Moreover, when we find it *combined in definite quantities* with different forms of matter, and the moment it is released, the matter assuming a *new form*, I think we are fairly entitled to predicate of it as having substance—or, at all events, subject to the higher laws of matter. It may be a *substance* standing infinitely higher in the scale of levity to hydrogen, than that gas does to platinum; seeing it scarcely follows, that because we have not instruments capable of detecting its ponderability, that it is absolutely imponderable. Again, by experiment, we find that the quantity of electricity which is naturally associated with particles of matter, gives them *their powers of combination*; and that the same power, when *condensed* into a current,

causes them to assume other forms. Yet there is, says Faraday, "such contradictory evidence, such equilibrium of opinion, such variation and combination of theory, as leaves us in complete doubt, respecting which we should accept as the true interpretation of nature."

This gentleman has also made a series of most laborious researches, with a view to establish the identity of the electricities, or, in other words, to prove that the electricity of the machine, of the atmosphere, of the magnet, &c., and that of the voltaic pile, are one and the same, although developed under different circumstances.

It would be foreign to my present purpose to enter here into anything like a discussion of this question, or to repeat the views of Faraday. I am inclined, however, with diffidence, to look on them as partially inconclusive. Electricity, *per se*, has perhaps never yet acknowledged the control of man, nor can there be a doubt of *its* homogeneity; but those apparent modifications of it that we find so intimately commingled with matter, are, no doubt, subject to *different* laws, arising out of the different forms of the matter they are associated with. It may be eliminated from some bodies in a comparatively fluid state; in others, in one analogous to the gaseous. Thus the electricity developed during the oxidation of zinc, has properties very different from that developed by friction on a piece of glass. It is true, they have many in common; so have the acids; but it is the few properties they have *not* in common, that establishes their want of identity. Thus the most delicate gold leaf electrometer, the suspended leaves of which will be diverged considerably, by merely holding an excited stick of sealing wax within a few inches of the condensing plate,—will remain unmoved by *absolute contact* of the poles of a voltaic arrangement, sufficiently powerful to *fuse* platinum wire. I am aware this has been attempted to be explained, but I think unsatisfactorily. I am, however, bound to mention the experiment often made in proof of this position. Take a perfectly clean disc of copper, and one of zinc, each three or four inches in diameter; and fasten with a piece of soft cement, a glass handle in the centre of each. If the copper and zinc are held by these insulating handles, and pressed together gently, and *one* of them be afterwards brought into contact with the condensing plate of the electrometer, the same operation being repeated several times, we shall find, on bringing the condensing plate into contact with the other plate of the instrument, that the gold leaves are *sensibly affected*, and indicate positive or negative states of electricity, according to the metal which is placed in contact with the condensing plate. This experiment, however, proves nothing, because the electricity thus obtained may be extracted from the surrounding atmosphere, by the slight percussion of a stratum of air necessarily interposed between the copper and zinc plates.

Becquerel has also shown us, that most substances, if thus brought together while insulated, will exhibit signs of positive and negative electricity. If one of them be elastic, the more unequivocal will be the results. Indeed, so general is this law, that if a cork be pressed

against the skin of an orange, the cork becomes *positive*, while the skin is rendered negative; while, on the other hand, if the orange skin be pressed against a disc of caoutchouc, a difference in the electrical states of these two bodies will become apparent. Semi-liquid substances, if compressible, give similar results. This philosopher, in his valuable work, has also shown us, that electricity is evolved by the cleavage of minerals. If a plate of mica be cleaved in the dark, we perceive a slight phosphorescence; it may then be ascertained that the two pieces are in contrary states of electricity. In fact, these and similar experiments have rendered it more than probable, that it is impossible to bring *two substances of any* description together, without disturbing their electrical equilibrium, and thereby causing a developement of electricity.

Mr. Faraday has suggested, what may be termed a new electrical nomenclature; a portion of it, however, is somewhat hypothetical; notwithstanding which, in the present state of our knowledge, it is highly useful, and, to a certain extent, it may be expedient to retain it. In what follows, I shall adopt the portion of it that appears least questionable. For instance, it was usual to call the terminal wires of the battery, its poles: it is now all but agreed among electricians, to term them *electrodes*; one being the positive electrode, the other being the negative electrode. Again, any fluid body capable of being decomposed by a simple voltaic current, is termed an *electrolyte*. Thus, in the experiments to follow, sulphate of copper *in solution* is an electrolyte, although not *directly* decomposed by the current; or when water is decomposed, *it* also becomes an electrolyte, and its two constituent gases are *ions*, should they be eliminated *by* electro chemical action, as at present understood. Thus, the *ion* hydrogen will be found liberated from the *electrolyte* water, at the *negative electrode* of the battery; and the *ion* oxygen will also be found at the *positive electrode*. But if *one* of the *ions not* in combination with another be subjected to the electric current, it will remain perfectly neutral, and not be found at either of the electrodes. And again, if an *ion* be found at one electrode, another *ion* must also be found at the other. I shall not further embarrass my readers with more of these novel but useful terms; my present object being rather to familiarise them with the application of facts already known, than to lead them far into the depths of what may be justly termed, the metaphysical portions of the science.

With this brief preliminary, I shall detail a few of the most striking experiments connected with metallic and mineral precipitation, as they will form an appropriate introduction to the main object of this treatise.

If we dip a piece of clean iron into a fluid containing a salt of copper in solution, and let it remain for a few minutes, it will be found to have received a coating of pure copper. In all probability, this fact was known as early as the first copper mine, the waters of which generally contain some cupreous salt in solution. It is easy to imagine

that the first piece of iron, so coming in contact, would make known this fact, and there can be little doubt that this was the first observed instance of metallic precipitation. It is but recently that this phenomenon has been fully recognised to be due to electrical action. Advantage, however, is taken of it in mining districts to obtain the copper contained in their waters, by immersing pieces of iron, and a nearly equivalent quantity of copper is obtained, in the form of a brittle deposit.

The reason why the copper is not deposited in the solid compact state, in this instance, may be explained by relating what in all probability takes place. If the iron, when first immersed, became *at once thoroughly* coated with the cupreous deposit, and this *not* dependent on any oxidation on its part, a *solid*, but excessively thin metallic coating would be the result, and the piece of iron thus cased would act precisely as a *piece of copper* under similar circumstances, which, I need hardly say, would have no further action on the solution. But such *not* being the result, let us see what really does take place. No sooner is the iron immersed in the cupreous solution, than *oxidation* takes place on portions of *its* surface; this is the primary action; the secondary being, the *deoxidation* of the copper held in solution; this, when set free, at once obeys the laws of elective affinity, and, while *under electrical influence*, attaches itself to the nearest *metallic* surface. The iron being only partially oxidised, presents to the copper a surface covered with metallic points, to these it attaches itself; but the oxidising process continues *still going forward*, in the interstitial spaces that have *not* been covered with copper; but as this proceeds, the prominent points get undermined, and, falling to the bottom, expose fresh surfaces again to be deposited on, so long as there is a piece of *metallic* iron left. Should cast iron, or steel, be so acted on, each having a proportionally large amount of carbon in combination, in such cases the iron is *displaced* by friable copper, but the *shape* of the original piece is still maintained by the strength of the skeleton of carbon, which is not acted on. The primary action in this, and in most other instances of electro-chemical decomposition, is caused by the decomposition of water, the oxygen of which combines with the iron. The secondary action being induced by the *nascent* hydrogen which separates the copper in the pure state from the acid, or compound radical,* with which it is in combination.

There can be little doubt that this explanation is equally applicable to the fossilification of many animal and vegetable remains. The process may be termed electrical, or chemical assimilation.

Chemists frequently make use of the term, "while in the nascent state;" meaning thereby, that gases and other elementary bodies possess properties *very different, at the moment of their birth*, or liberation, to those that they are known to possess afterwards. We have a notable instance of this in the volatile alkali, ammonia. If we take the pro-

* I allude here to the new views respecting the composition of the acids.

portionals of the two gases of which this substance is formed, and place them in a vessel *together*, we shall not be able thus to *form* ammonia. But, on the contrary, if we *generate* these gases simultaneously, *and in juxtaposition*, they will then combine in their atomic proportions, and ammonia will be the result. In some instances, one gas in the nascent state will be sufficient.

The obvious inference to be drawn from this fact, is that atoms of bodies at their *birth*, or chemical liberation, may be saturated with definite proportionals of electricity, and if, *while in that state*, they come into close contact* with other bodies *equally* saturated with their definite proportionals, and bearing the relation of positive and negative to each other, a chemical combination will take place, and a new compound be the result. The gases produced by the electric decomposition of water, do not form an exception to this rule; there being a peculiar odour attendant on their simultaneous production, which bears strong evidence of the formation of *another* substance. If we mix those gases, however, in similar proportions, but generated under different circumstances, we fail to perceive this peculiar olfactory sensation. I have kept the gases produced by the electrolysation of water, in well stoppered jars, for a period of eighteen months, when they still retained this property.†

From these facts it would appear, that if atoms of *nascent* bodies are suffered to remain uncombined for a sensible period, they become *less* saturated with electricity than they are at the period of their liberation, and that their powers of chemical combination are thereby diminished.

The manner in which electricity is *given off* the respective atoms of nascent bodies, may account for the phenomena of induction, and the fact itself bears a close relation to that state of matter termed polarity.‡ I have ventured these remarks here, although somewhat hypothetical, as the subsequent experimental illustrations are wholly dependent on the mutual action and re-action of electrical forces.

We have seen that iron does precipitate copper in the *metallic* state, and that the phenomena is electrical. I shall now adduce a further and

* It is demonstrable that there is no absolute contact; and, according to the ingenious experiments of Döbereiner with spongy platinum, the closer atoms of bodies are brought together, the more readily combination will take place. Thus, oxygen and hydrogen were made to combine to form water, by being brought together in that highly comminuted substance. It has also been employed by Dr. Hare, of Philadelphia, to form ammonia. Thus, atoms of *nascent* bodies, bearing the electrical relations of positive and negative to each other, are brought into more immediate contact *while in that state*, and thus induce combination.

† I perceive from the "Transactions of the Royal Society," that Professor Schoenbein, of Bâle, is making researches on this subject, and that he has met with some conclusive results.

‡ The atoms of *recently* decomposed bodies may be readily supposed to carry off free electricity with them. We daily perceive an analogous instance of this, in vapour carrying off heat; itself, in all probability, but another modification of electricity.

far more striking illustration of this latter fact, and one calculated to throw some additional light on geology and the sister science of mineralogy. Procure a tall test glass or tube, fig 1. Let it be of small interior diameter, three-fourths to an inch, and five or six inches high. Fill it up to the dotted line with a saturated solution of sulphate or nitrate of copper, and then add as much pure water, to which a few drops of nitric acid has been added, as will nearly fill the glass. The difficulty in performing this experiment is to pour the water in the tube in such a manner as to prevent it *intermingling* with the cupreous solution beneath. A dexterous manipulator will perform this with the dropping tube alone, but it is accomplished with much more certainty by employing a thin disc of cork, smaller than the interior diameter of the test glass. Let this float on the surface of the cupreous solution, and then drop, with a tube, the acidulated water gradually on the surface of the cork. It will continue to float until it reaches the top, when it may be removed. If this is manipulated with common care, the water will be found to float on the metallic solution, the density of the one being greater than the other. This being accomplished, take a thin slip of copper, *a*, let it be about six inches long, and one-fourth of an inch wide, brighten it with glass paper, or by dipping into dilute nitric acid. When so treated, immerse it in the test glass, resting it on the bottom; one portion of it will thus be immersed in the cupreous solution, the other in the acidulated water,—the difference of colour displayed by the two solutions, will at once indicate whether the separation is complete. It must now be suffered to remain at rest for eight or ten days—if shaded from light, so much the better. At the end of that time it will be found that crystals of *pure copper* have formed on that portion of the slip immersed in the cupreous solution, while the portion immersed in the acidulated water has become black and oxidised, and *another portion*, equal to one-eighth of an inch that occurs just at the junction of the fluids, has remained untouched.

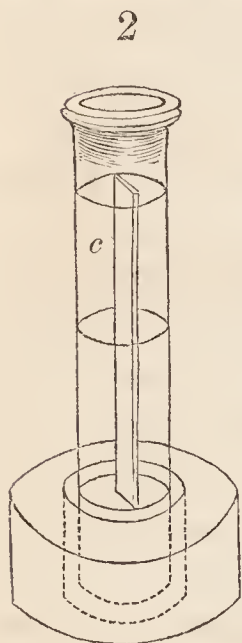


This experiment may be so varied as to cause most of the metals to *precipitate themselves* from their respective solutions,—the instructions given to perform the above experiment, applying equally to all. The necessary conditions being a saturated solution of any of their salts, the sulphates or nitrates performing best; next, water containing a few drops of the acid that most readily *oxidises* the metal to be acted on, and this *carefully* superimposed on the solution; and, finally, brightened slips of the respective metals. If the action is slow, the metallic crystals thus formed, when viewed with the microscope, have all the beauty and perfect crystalline structure of those formed by similar agency in the great laboratory of nature. The necessary reason assigned by electricians for slow action in these experiments, is to allow the respective molecules *time* to arrange themselves according to the laws that govern their respective forms of crystallisation.

In these and similar experiments it is worthy of remark, that for every portion of metallic deposition that takes place on the negative end of the slip of metal, we have an equivalent amount of oxidation on the other or positive end.

The fact, that the precipitation of one metal by another was due to electricity, was suggested as early as 1800 by Ritter and Sylvester, and some of the above experiments were realised in 1806 by Bucholz, but without his drawing any inference as to their probable connection with mineral formations. That this must be the case, subsequent experiments have proved beyond doubt; for we now find, that we are able not only to precipitate the metals in a crystalline form, but their oxides and deutoxides, in the precise form in which we find them in nature.

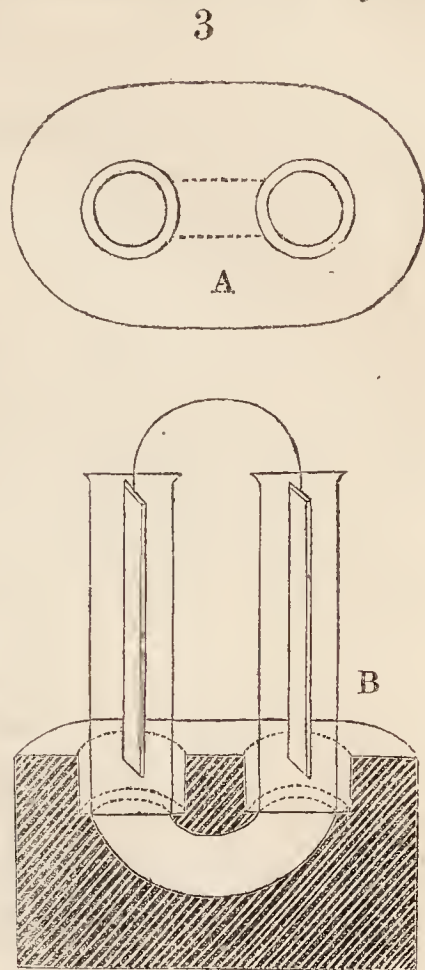
To obtain crystals of protoxide of copper, procure a glass tube closed at one end, and having a stand formed of a block of wood, as in fig. 2, into which the lower end will fit. A portion of deutoxide of copper must now be put in the tube, after which a saturated solution of nitrate of copper, until nearly full. A brightened slip of copper, *c*, must also be added, long enough to reach to the top of the fluid, and to touch the bottom of the tube. The upper end of the tube must now be hermetically sealed, with a piece of cork and soft cement, and the apparatus suffered to rest for ten or twelve days, at the end of which time it will be perceived that small bright octahedral crystals, of a deep red colour, have been formed on that end of the slip of copper not in contact with the oxide previously placed in the bottom of the glass. These crystals are protoxide of copper.



To obtain crystallised protoxide of lead, procure a tube apparatus like fig. 2, and put powdered litharge at the bottom of it, to the thickness of half an inch: pour over this a diluted solution of subacetate of lead, and plunge a slip of lead into the fluid, bringing it in contact with the litharge. Let the tube be sealed, as in the former experiment. In a few days the surface of the lead becomes covered with very small prismatic needles of hydrate of lead; occasionally the lead is reduced; and sometimes there occurs a deposit of dodecahedral crystals of protoxide with pentagonal faces; these lose their transparency when in contact with atmospheric air.

To procure a crystallised oxide of zinc, employ the apparatus fig. 3. It consists of an earthenware stand, containing two round holes, into which tubes may be adjusted. These are connected together by a tunnel, which may be filled by moist clay or plaster of Paris. This apparatus will be found particularly useful for these experiments. A, is a plan of the stand, showing the apertures in which the tubes are to

be placed. B, is a section of the same instrument, showing the tunnel and the glass tubes in their places, with the electrodes connected by an arc of wire. This apparatus may be made of any size. Into one of the tubes put a solution of zinc in potash, in the other put a solution of nitrate of copper, the communication having been made by the plaster of paris in the small tunnel, which should be previously moistened with nitrate of potash in solution. If the tubes are large, say six or seven inches high, and one inch and a half in diameter, it will be found sufficient to immerse a piece of lead in the zinc solution, which may be connected by the arc of copper wire to a plate of copper, which is to be placed in the cupreous solution; but if a much smaller apparatus is used, it will be necessary to connect the slip of lead to the positive electrode of a small battery, and the copper to the negative one. When this experiment has been in action for a few days, there will be found on the plate of lead small flat prismatic crystals, disposed in the form of stars. These crystals are oxide of zinc, as may be proved by subjecting them to the action of acetic acid, when they produce acetate of zinc, from an aqueous solution of which sulphuret of potassium throws down a white precipitate.



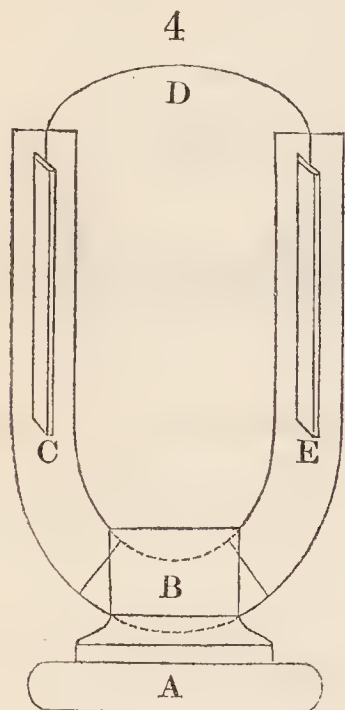
Numerous and instructive electro-chemical decompositions and compositions may, by these means, be made. There seems to be no assignable limit to such experiments, and when we view them as an epitome of what is taking place in the vast laboratory of nature, they become doubly interesting. It would not accord with the limits of this publication to detail all that have been already made, in the closet of the experimentalist,* illustrative of this universal principle of nature. I shall, therefore, only lay down some general instructions, and describe a simple form of apparatus by which they may be readily performed.

It seems to be a principle in nature, that substances are solid or durable, in the ratio of the time occupied in their formation. In the vegetable world this is more particularly evident. Wood of trees requiring the longest amount of time to arrive at maturity, is of a much harder and solid nature than the wood of those having a quicker growth. In like manner electro-chemical products that are combined by the action of currents of intense energy, are brittle or friable, and possess less cohesiveness between their secondary atoms, than similar

* For a more complete account of the chemical effects of electric currents of low tension, I would refer to Becquerell's work, *Traité de l'Electricité*, or to a most interesting chapter of it translated in "Scientific Memoirs."

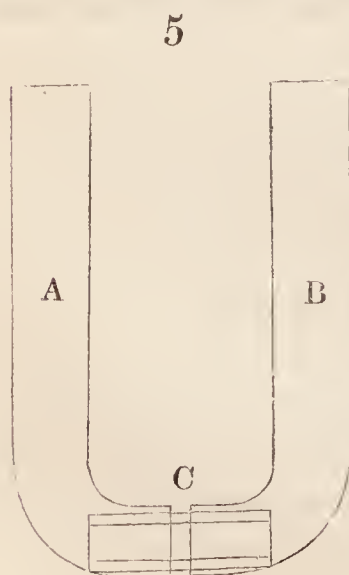
compounds formed by slow currents of low tension. Crystallisation would appear to be governed by a similar law. When we evaporate a solution of any salt rapidly by means of heat, minute and friable crystals are generally the result; but if, on the contrary, a similar fluid is submitted to evaporation at a lower temperature, the crystals are proportionably solid and large, as if *time had been allowed for them to assume their natural form*. In these instances, there can be little doubt that the *same* agent is actively at work in *all*. Bearing the above in view, the apparatus for conducting these experiments successfully, must be of such a nature as not to act too powerfully on the substances to be operated on. By powerful currents, the decomposed products become isolated or repelled, and cohesion is rendered difficult, if not impossible.

The most simple form of apparatus for these purposes, is a bent glass tube, fig. 4. It may be of an inch diameter, and each of its limbs five or six inches long. For convenience, it may be fixed on the turned wooden stand, A. B represents a division in its interior, made by pouring into it plaster of paris. When this has become solid, different solutions of substances required to be acted on, may then be placed in each compartment thus obtained.



The solutions thus disposed may be either submitted to the action of a battery, by immersing in them slips of platinum connected with the respective poles: or electric action may be induced by a simple voltaic pair, connected together by an arc of copper, platinum, or silver wire, disposed as in the figure. C may represent a slip of copper or platinum, D the arc of wire, E a slip of zinc or lead, either of which will act as a positive electrode in this arrangement.

The action is regulated by the thickness and texture of the plaster partition B, and also by the *distance* that the lower ends of the electrodes are from each other. In using this apparatus, I have often experienced a difficulty in regulating the thickness of the division, and also in readily removing it when required. This inconvenience necessarily arises out of the shape of the tube. To remedy this difficulty as much as possible, I have had tubes bent at right angles, as in fig. 5.



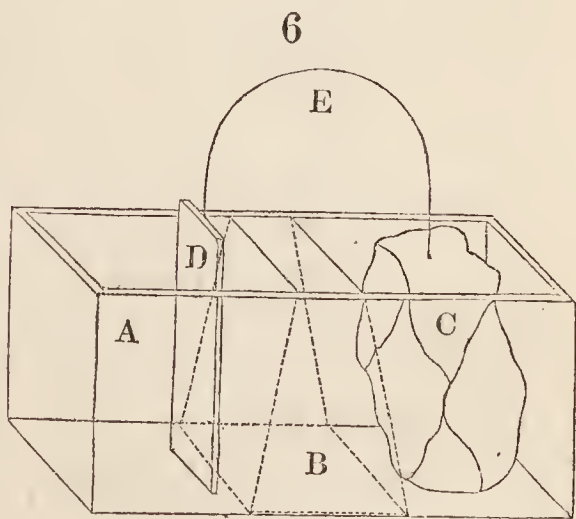
A and B represent the tubes, C a *good* cork, which, after being fitted to the tubes, is bored with a cork borer. The aperture thus formed, is then filled with plaster of paris, moistened with any other substance we may be desirous of acting on. It is then inserted into the ends of the tubes, which being brought together, will be found perfectly

water tight.* By these means we may easily regulate the thickness of the division, by the length of the cork tube, and it is readily removed when required to be examined, which is, occasionally, of the utmost importance, or should the apparatus be required for another experiment. With such an instrument, most of the important electro-chemical experiments may be made, it being not only simple, but convenient, as we may operate on small quantities, and be allowed from time to time to examine the results, without disturbing the necessary repose of the experiment itself.

A simple form of apparatus has been used by Mr. Fox, to perform the very important experiments which have thrown so much light on mineralogical phenomena, but more particularly the formation of native copper, so frequently occurring in the mines. These interesting appearances owe their principal interest to the fact of the occurrence of pure copper in contact with the black and grey ores, which contain less than fifty per cent. of metallic copper. The pure copper, however, occurs in small branches of crystals, ramified into a sort of filligree work, and frequently on the outside of the poorer ores.

Mr. Fox being practically conversant with these facts, happily conceived the idea that they might be due to electricity, currents of which he had previously detected traversing the mines, by a judicious arrangement of galvanometers.

To put this philosophic hypothesis to the test of experiment, he contrived an apparatus that may be imitated by observing the following directions:—A, fig. 6, represents a small earthenware trough, of a capacity sufficient to contain from a pint to a quart of liquid. This is divided into two compartments by the clay or plaster of paris partition B.† This is firmly pressed down into the trough, so that the resulting cells may be water tight; a piece of sulphuret of copper, C, is attached to a copper wire, E, to which has previously been attached a piece of zinc, D. When this arrangement is completed, and adjusted in the cells, as in the figure, pour into the cell containing the sulphuret of copper, a saturated solution of the sulphate of copper (blue vitriol): into the other cell, containing the zinc, pour a weak solution of common salt. In the course of three weeks, the piece



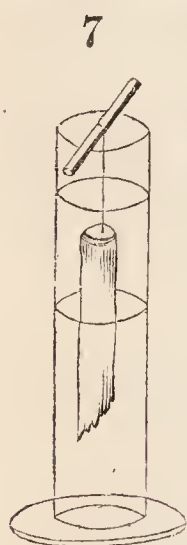
* A little sealing wax or resinous cement may be applied to the junction should the fitting not be considered good, but this in all cases *before* the fluids are put into the tubes.

† In the whole of these experiments the object of *porous* partitions is, I hope, obvious: it being, to furnish a medium that will separate the two fluids, and at the same time form a slow conductor for the electric action.

of ore will undergo a series of changes. In a short time it becomes beautifully iridescent, assuming the semblance of the "peacock" copper ore, shortly afterwards it passes into the purple ore, termed by the German miners "Buntkupfererz," and it ultimately passes into the grey copper ore. This is not a mere superficial change, but penetrates some depth into the substance. While these changes are going on, metallic copper, in brilliant octahedral crystals, is deposited on the surface of the ore, the copper thus formed being in every respect similar to that formed by nature.

As a still further proof that the ores themselves are in all respects sufficient to generate electric action equal to the production of the observed phenomena, another and a still more conclusive experiment was made. It was in every respect similar to the foregoing, with this exception, that instead of the zinc, a piece of the grey carbonate of copper was used as a positive electrode, and nearly similar results were the consequence; thus proving to us beyond doubt, that nature possesses within herself this great active agent, necessarily arising out of the action and reaction of her own elements.

To prove that still more simple materials if possible, would produce similar effects, I made the following experiment:—I procured a tall test glass, fig. 7, and filled it with two fluids in the manner already pointed out, the lighter fluid being superimposed on the denser one; this being done, a long narrow piece of *sulphuret* of copper was procured, and fastened in the glass in such a manner as to allow a portion of it to be in both fluids. A few drops of nitric acid were added to the water; this was kept in a dark place, and at the end of a month crystals of pure copper had begun to form on the portion of the sulphuret immersed in the solution of the sulphate of copper in the bottom of the glass, while that portion of it in contact with the acidulated water had undergone the change already pointed out.



Another experiment, illustrative of the formation of metallic veins, was made. A tall cylindrical decanting glass (fig. 8,) was taken, six or seven inches high, and two inches diameter; in its lower end was placed a disc of zinc, A, having a little less diameter than the interior of the vessel. Over the zinc was poured a stratum of plaster of Paris, B, having mixed with it sulphate of copper, in solution; this being allowed to set, crystals of the sulphate of copper, C, were then added, and the vessel nearly filled with pure water. It was then covered to prevent evaporation, and allowed to remain eight days, at the end of which time, more crystals of the sulphate were added, the former having disappeared. It was now allowed to remain uncovered, and the solution suffered gradually to evaporate. At the lapse of a month from the period of putting the experiment in operation, the solution had entirely disappeared: the glass was then broken, and the interior of the plug of plaster was examined, by cutting it perpendicularly in halves with a

fine saw. A most beautiful sight presented itself. The metallic zinc had almost disappeared, and a sulphate of zinc had been formed immediately over it; above this, and in the body of the plaster, a series of veins of pure metallic copper was found ramified through every portion of the interior. In fact, it presented a miniature representation of the metalliferous veins of one of the richest mines. In an electro-chemical point of view, it may be interesting to explain what takes place during the continuance of this experiment.

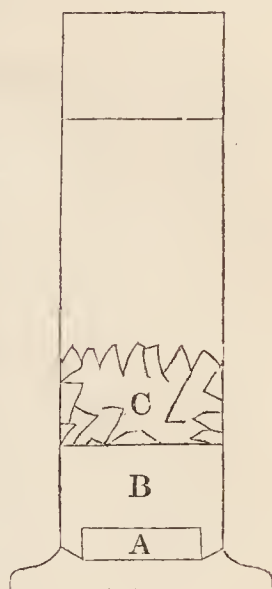
Water is decomposed by the action of the disc of zinc, oxidation taking place on its surface, and an equivalent of hydrogen is necessarily set free. An oxide of zinc is *the first* result of the decomposition of the water; but, according to the electro-chemical theory, a *definite portion* of electricity *must at the same time* be given off. Let us now trace its action and agency.

No sooner is the electricity liberated, than it decomposes the sulphate of copper, setting free sulphuric acid and oxide of copper. The *nascent* hydrogen now acts on the oxide of copper, and effects its deoxidation. To reduce the copper, one other condition is necessary, which is, that the action shall take place *in contact* with one of the electrodes. In this instance it does take place in contact with the unoxidised portions of the zinc, and, consequently, the crystalline copper forms on its metallic surface. The sulphuric acid set free, must now be accounted for. It combines with the oxide of zinc, formed in the first instance, and we have as the product of this last combination, the salt, sulphate of zinc.

In some cases, another product is obtained. In using the zinc of commerce, a portion of iron is generally combined with it, which is set at liberty as an oxide; this combines in very small proportions with zinc and sulphur, to form a substance analogous to the blende of the mines, or sulphuret of zinc. In this case, the sulphur must be obtained by the decomposition of the acid itself; and I am confirmed in this opinion by the fact, that a quantity of oxygen is found at *the bottom* of the vessel, upheaving the plug containing the zinc and the whole of the supernatant fluid, and causing it to run over the edge of the glass. I have sometimes placed a rod of glass to bear perpendicularly on the plaster of paris, and caused it to support a weight of 20lb. on its upper end, notwithstanding which, the whole was upheaved with the greatest ease; and its power must have been in reality much greater, as the cohesive force exerted by the plaster on the glass was also destroyed, which is by no means slight. When the gases* were set at liberty, the plaster disc would quietly regain its former position at the bottom of the vessel, again to be upheaved by another formation.

* Sometimes an explosive mixture of the gases is formed. This may be ascertained by pressing the mass down towards the bottom of the vessel, and exploding with a lighted taper the bubbles that rise to the top of the fluid.

8



It is impossible to help connecting the whole effects of this experiment with those phenomena that geological observation makes known to us, as periodically taking place in several portions of the crust of our earth; but the last result is more particularly deserving our notice, as it would appear to offer an easy and natural solution of the much-questioned phenomena of the gradual upheaving of land, and its equally gradual subsidence.

This would more particularly apply to the remarkable case of the temple of Jupiter Serapis, at Puzzuoli, on the coast of Italy. This celebrated temple affords the most conclusive geological evidence, that the land on which it rests has been repeatedly submerged, and again raised from the bosom of the waters. The shafts of some of its columns are at present above the level of the sea; they remain, however, *in situ*, standing as perpendicular as when placed there at the command of the architect. "On these," says Dr. Mantell, "are inscriptions, not traced by the Greeks and Romans, but by some of the simplest forms of animal existence, which have here left enduring records of the physical changes that have taken place on these shores, since man erected the temple in honour of his gods. The tallest column is forty-two feet in height; its surface is smooth and uninjured to an elevation of about twelve feet from the pedestal, where a band of perforations, made by a species of marine boring muscle, commences, and extends to a height of nine feet, above which all traces of their ravages disappear."* Mr. Babbage attributes the whole of these phenomena to the expansion of the lower strata by heat; and contraction, on the other hand, by refrigeration. This explanation presents us with many difficulties. Should the heat necessary to cause this amount of expansion be admitted, it must be evident it would cause the rock to expand *laterally*, as well as perpendicularly; and unless this is shown to have taken place, the explanation is not complete. But if we adopt the hypothesis that these phenomena are sometimes the result of electro-chemical action, the solution is easy, while it is consistent with observed facts. Heat also is necessarily evolved during chemical decomposition, however slow; and, no doubt, it is retained in larger amount, as we recede from the surface, from the difficulty of refrigeration. Gases to a greater or less extent are given off. Wherever they chance to commingle in the proportions proper to form water, a highly explosive mixture is the result. They may explode from various causes,—the contact of heat, or of metals in a peculiar state of subdivision, or the descent of lightning to the earth,—or they may gradually escape. Should the former take place, we have a slight shock of an earthquake, or a tumbling in of the land; should the latter prevail, a gradual depression of the already upheaved strata will be the necessary consequence.

In the present state of our knowledge, it would be carrying the electro-chemical theory perhaps too far, to imagine that *all* such phenomena

* See Lyell's Geology, and the observations of Mr. Babbage.

are immediately due to like causes; but where we find the rising and alternate falling of strata so slowly progressive as to allow the unsupported shafts of immense columns to remain in their places, surely we are justified in doubting a theory which we necessarily associate with convulsions of the most fearful kind.

Another experiment was made by Mr. Fox, which has done much to illustrate and explain another geological problem. It is an admitted axiom in the science, that stratified rocks are sedimentary; that is to say, they have been deposited at the bottom of a primeval ocean, or lake, in a horizontal position; and wherever we find a departure from this principle, it is to be attributed to an upheaving, or a lateral force; a force from *above* being inadmissible. The great exception to this rule exists in the slate formations. In them, it is found that there are *two* species of apparent stratification, one being at nearly *right angles to the other*. The one is termed the lines of lamination, the other those of cleavage. The first could be easily accounted for, on the hypothesis of a horizontal deposition at the bottom of the waters; but the fact of the *cleavage* of these rocks being generally perpendicular to the first, was not so easily explained. Volcanic heat was, as usual, called to aid the geologist, but without affording conclusive evidence.

Mr. Fox, however, thought them due to electricity; and to put it to the test, he made an experiment with an apparatus somewhat similar to the one described by fig. 6. The difference consisted in having a piece of copper plate substituted for the nodule of sulphuret of copper, mentioned in his former experiment, and salt and water was put into *both* cells, a clay wall separating them as before. After the lapse of from three to four months, it was found that the interposed wall of clay had become *laminated*, or split into thin plates. The result of this experiment presents us with an easy solution of the cause of these anomalous appearances in the slate formations, they being chiefly composed of alumina, or clay. Several who have repeated this experiment, have been disappointed in obtaining similar results. The author, in a repetition of it, obtained the most unquestionable proof of its correctness, when the experiment had been continued for a considerable period, and the fluids suffered to evaporate gradually, until the interposed wall of clay had become dry. In making this and similar experiments, another interesting appearance may be observed, which has also been applied to solve a geological enigma, often observed by miners.

It sometimes occurs that mines are inundated by the workmen accidentally penetrating into walls of coarse argillaceous schist, which occasionally separate one portion from another. In such instances, it has been observed, that although a communication must have existed between the water of the inundation, and that already existing in the other portion of the mine, in consequence of the porous nature of the interposing medium, yet the water stood at different levels. Thus the water seems in these instances to disobey the general laws, by which the pressure of fluids is known to be governed. In electrical experi-

ments of low tension, and made with apparatus divided into cells by the interposition of porous substances, an appearance similar to this takes place; the fluid in the negative cell invariably attaining a higher level than that in the opposite compartment. Although this is known to be the fact, it has not been accounted for in a satisfactory manner; and as this appearance always accompanies the depositing processes described in another portion of the work, it may be interesting to enter into a few details respecting it.

In 1816, an experiment was made by Mr. Porret,* in cells divided by pieces of bladder, which seemed to be tolerably conclusive, and went far to show that this phenomenon was due to electricity, *per se*.

I must confess, moreover, that while reading an account of this gentleman's experiment, I could not do otherwise than admit the correctness of the conclusions he had come to, more especially when they were coupled with facts I myself had frequently observed under other circumstances. Mr. Porret's experiment was made in the following manner:—

He procured a small glass jar, of the capacity of about an ounce, and caused it to be cut perpendicularly into two equal parts. The rough edges were then ground, and a piece of bladder interposed, which was fastened by cement, and while it was hot, both halves of the glass were brought together. By these means a vessel was constructed, having a water-tight division. To prove this, he placed water in one cell, and having suffered it to remain at least for a few hours, *no* water was found to have passed into the other.

This being ascertained, the electrodes of a battery of 80 pairs of one and a quarter inch double plates, were introduced into the two cells. A few drops of water being placed in that which was previously empty. The *positive* electrode was placed in the cell that contained the largest amount of water.

The battery was now kept in action for nearly two hours, when it was found that water *had passed* from the positive cell into the negative, and that in this latter it stood nearly *an inch higher* than in the former, or, in other words, at the end of the experiment the circumstances of the cells were nearly reversed.

Mr. Porret tells us he has repeated this experiment, and always with similar results; but we are not told whether he observed decomposition of water to take place, or whether it was distilled, although it was said to be "pure water." The alteration of any of these conditions would materially alter the conclusions to be drawn from the fact.

That I might be able to record the fact from personal observation, I determined to repeat the experiment, but have been unable to obtain results similar to those described by Mr. Porret.

I operated *on distilled water*, with batteries of various power, in cells separated by porous diaphragms of different substances: still the water in

* See Thomson's Annals for 1816.

each retained its natural level. This gentleman may have omitted to mention some of the conditions necessary to its success; I, however, still consider it an interesting subject of research.

Thus finding, as far as my own experience went, that where pure water only is operated on, this difference of level is not perceptible; but on the other hand, knowing that it invariably takes place should the fluids in the cells be solutions of *any of the salts* usually acted on; I have in consequence been recently induced to make a series of experiments to determine this question, and the results appear to me satisfactory. I shall not detail them here, further than to show how the fact may be demonstrated.

Take a bent glass tube, such as fig. 4, but without the stand. Pour a few drops of water in it, just sufficient to stop the communication at the bend. Let the bent part containing the water be dipped in any freezing mixture, and when the water is converted into ice, pour a saturated solution of sulphate of copper into one limb of the apparatus, and a saturated solution of sulphate of zinc into the other. Let both liquids stand at the same level, and within an inch of the top. The division of ice must now be thawed, when immediately it will be found, that the cupreous solution becomes elevated, while the solution in the opposite compartment is proportionally depressed, the difference of level being nearly an inch. The explanation is now not difficult, the specific gravity of the cupreous solution being much less than that of the zinc. To render this still more apparent, if the experiment is repeated, and before the ice is thawed, let a few drops of sulphuric acid be added to the sulphate of copper, to bring the fluids to the same specific gravity. On thawing the ice, we shall then find that both fluids stand at the same level; or if, in the first instance, a few drops of the acid are added, the fluids will regain their level. Should a small portion of mercury be placed in the bend of the tube, the results will be slightly apparent.

These experiments have been repeated with other fluids, and always with the same results. If these are now applied to what we know takes place during electro-chemical decomposition, the phenomenon is readily explained. If, for instance, we place a saturated solution of a salt of copper in one leg of the tube, and in the other *weak* salt and water, we shall find in this case, that the cupreous solution stands *lowest*. But if we now take a pair of *equal* sized slips of zinc, and copper connected together by an arc of copper wire, and insert the copper in the cupreous solution, and the zinc in the salt and water, we shall find in a few hours, that the relative level of the fluids is beginning to change, and that ultimately the fluid in the limb containing the slip of copper, will stand nearly an inch above the other. Let us now inquire what has taken place to cause the change. The sulphate of copper has become decomposed, and sulphuric acid has been transferred to the opposite cell, where it combined with the oxide of zinc to form a sulphate of zinc—a salt the solubility of which is 1.4, while 0.33 is that of the sulphate of copper. Consequently, the difference of solubility in these salts neces-

sarily gives rise to a difference of specific gravity in each of their solutions, both being sulphates.

In the depositing processes this difference of specific gravity takes place. If, for instance, the gas glass apparatus be used for the purpose of depositing copper, and the fluids in each of its compartments be placed at equal levels at the commencement; it is clear that, from the moment action has commenced, this state of things cannot continue, but must gradually begin to change the specific gravity of the fluids, and thereby their relative levels. That is, the copper contained in the cupreous salt being reduced to the metallic state, that portion of the fluid is thereby rendered lighter; while, in the other cell, the electro-chemical action has a tendency exactly the reverse, the oxide of zinc continuously formed, combines with the sulphuric acid to form sulphate of zinc, as in the instance already given.

In the course of geological observation, I have often observed apparently anomalous appearances that occur in clay formations. In them concentric rings, as it were, of stratification, are sometimes seen: some instances are, however, so contorted, as to have led to the opinion that they were due to lateral pressure while the rocks were in a state of semi-liquefaction, or softness, caused by their primitive water. There are, however, cases where this method of explanation is unsatisfactory, as where they appear in concentric rings without any apparent disturbance.

Subsequent to Mr. Fox's experiment on the lamination of clay, I made several experiments with a view to find a solution of these enigmatical appearances, but whether the one now given is correct or not, I leave better informed geologists to determine.

In the course of my electro-chemical experiments, I have always observed, where observation was possible, that the current of electricity traversed a fluid, or any good conducting substance, in right lines; but where an inferior conducting substance occurs, it assumes the form of its surface, or if it pass through the substance, it does so in a curved or zig-zag direction. This conclusion was arrived at inferentially, by observing the lines of crystallisation caused by the electro-chemical force when the action is slow. If, then, we admit that electric currents pass through clay slate rocks while in the wet state, and if we also take into consideration the highly comminuted state of the particles of the pulpy mass, we shall then have no difficulty in accounting as before for the cleavage occurring in these formations in comparatively right lines.

But if we now suppose the interposition of boulder, or mass of any hard or worse conducting substance, to intercept the passage of the electric current, it is easy to imagine what would take place. The line of the current would then assume the form of the surface of the interposing mass, and the exterior coatings of clay would have a cleavage in accordance with that form. Being desirous to put this hypothetical inference to the test of experiment, as far as circumstances would permit, I proceeded as follows:—

I procured some very fine pipe-clay, and having ground it to powder, I moistened it with a weak solution of nitrate of potass, until it had assumed the consistency of clay, as we find it in the natural state. I then inserted, in the interior of the mass, several rounded pebbles, and placed the whole so prepared, in an apparatus described in fig. 6,—the prepared clay forming the interposing wall that divided the vessel into two compartments, in each of which were placed the terminals of an electromotive pair, being plates of copper and zinc connected by an arc of copper wire, as in the former experiment. The arrangement was excited by salt and water in each cell. The action was kept up for five or six months, by occasionally adding water to replace that lost by evaporation. At the end of this period, the plate of zinc had become dissolved, and the whole was suffered to dry. On examination, it was found that right lines of lamination had been formed where the clay only occurred, but where the pebbles were interposed, the laminæ had assumed the shape of each pebble, as if the electric force had traversed exactly in the lines of their surface. Fig. 9 will represent a section of the clay at the termination of this experiment. On examining the laminæ with a lens, minute crystals of a salt of potass were found on their surface.

9



The researches made by Mr. Crosse on electro-chemical synthesis of minerals, must now come under brief notice. This gentleman has employed himself for a very considerable period in making these most interesting experiments, on a very large scale; and it is more than probable, that had circumstances induced him to lay his discoveries before the public at the time they were made, a high claim to originality in this peculiar walk of philosophy, would have been awarded to him. As it is, they have been given to the world, at least synchronal with others, and from the scale on which they were made, the results have surpassed those made by other philosophers. In Germany, it is said, Professor Mitscherlich has succeeded in procuring crystals of some of the oxides, by means very different from those of Mr. Crosse; but this only shows us, that nature has different methods of attaining similar results. According to this chemist, when we are desirous of obtaining crystals by the agency of heat, the substances must be first intensely heated, and afterwards suffered to cool gradually. The metal titanium is also found in the *slag*, or porous refuse from iron furnaces, in an absolutely pure state, and crystallised in cubes.

It is worthy of remark, that, in such instances, the most perfect specimens are found in slags that have been suffered to accumulate untouched for a series of years. The most beautiful and perfect crystals of this kind that came under the author's notice, were found in a slag that had been unmoved for fifteen years. It would be hazardous to assert, in these and similar instances, that the crystals were not produced at the moment the surrounding mass began to cool; but it

would be still more hazardous to assert, that electricity was *not* the agent that induced crystalline forms in these cases, as we know it does under other circumstances. Experiments are, however, still wanting on this branch of inquiry, it being at present a mere matter of conjectural probability.

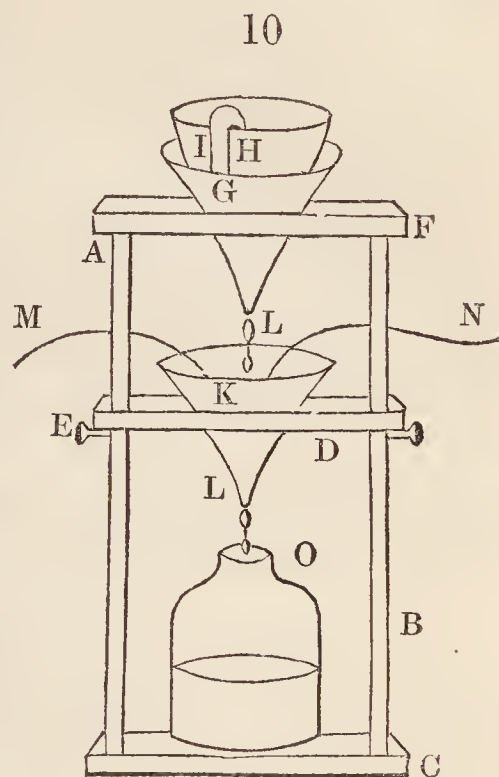
With respect to the electro-chemical experiments of Mr. Crosse, the public are not in possession of numerous details. It would appear that the attention of this gentleman was called to the subject, by observing, in a cavern situated in the neighbourhood of his residence in Somersetshire, that certain portions of the roof, which consisted of slate, was studded with crystals of arragonite, while other parts of it, consisting of lime-stone, was covered with crystals of common calcareous spar. He thought these dissimilar appearances were due to the agency of electricity, as developed in weak or slow currents; and having ascertained by examination, that the water which dripped from various places contained carbonate of lime, and the sulphate of lime in solution. To put this conjecture to the test of experiment, he procured a portion of the slate and the limestone, and having subjected them to long-continued voltaic action, he obtained on the lime-stone crystals of calcareous spar, and on the slate, crystals of arragonite, being another form of calcareous spar.* In performing his experiments, this gentleman employs apparatus of much greater magnitude than Becquerel and others pursuing similar researches, and, in consequence, has succeeded in producing substances much more analogous to, or perhaps identical with, those that are found by the mineralogist. Mr. Crosse holds the opinion, that electricity, or the property so called, not only pervades every natural substance, but probably ranges throughout all space. With this idea in view, his electrical arrangements are on a scale adapted, as near as the limits of his laboratory will admit, to correspond with what may be supposed to take place in nature; imitating, among other things, the dripping of waters, holding lime and silica in solution, so frequently occurring in caverns, in mountainous or mineral districts.

I shall here describe one of Mr. Crosse's experiments, which will suffice to explain many others, of course varying the substances employed:—

To imitate the natural and continuous dripping of water, the apparatus, fig. 10, is employed. A wooden frame of about two feet in height, consisting of four legs (B), proceeding from a shelf (C) at the bottom, supporting another at the top (F), and containing a third in the middle (D). Each of these shelves may be about seven inches square. The upper one is pierced with an aperture, in which is fixed a funnel, within which rests a quart basin (H) on a circular piece of mahogany placed within the funnel. When this basin is filled with a fluid, a strip of flannel (I) wetted with the same, is suspended over the edge of the basin and inside the funnel (G), which, acting as a syphon, conveys the fluid out of the basin, through the funnel, in successive drops (L). The

* See Jameson's Journal, No. 43. The account of this experiment is not very distinct.

middle shelf of the frame is likewise pierced with an aperture, in which is fixed a smaller funnel of glass (K), which may support a piece of any porous stone, this must be placed immediately under the dropping (L) of the upper funnel. This stone is then to be kept electrified by means of the two platinum wires (MN) or electrodes of a voltaic battery, which may have an indefinite number of pairs of plates. From twenty to thirty pairs of zinc and copper plates or cylinders, of a size that may be contained in pint gallipots, and these to be kept excited by water alone, or water slightly acidulated, containing $\frac{1}{500}$ th or $\frac{1}{600}$ th of sulphuric acid. The lower shelf supports a wide mouthed bottle (O), which collects the fluid as it descends, and from which it is poured back into the basin (H), when the latter becomes exhausted. The drip is kept constantly up, a little water being added occasionally to supply that lost by evaporation. In this instance, the reason why the battery is charged with cold water only, is, that *diffused intensity* may be obtained, and not *quantity*; or, in other words, that the action may be slow and continuous.



The difference between *quantity* and *intensity* of voltaic electricity, may be familiarly illustrated in the following manner:—If we subject fine platinum foil to the heat of a furnace, it will remain unaltered; but if we subject similar foil to a particular portion of the flame of a common candle, it will be fused. We have in the furnace a large quantity of heat diffused; but, in the candle, we have a small quantity only, but it is concentrated, perhaps focused, and thereby enabled to act with intensity on a given point. If we then imagine a number of such points of flame acting on a given point of surface, we can make the analogy more perfect.

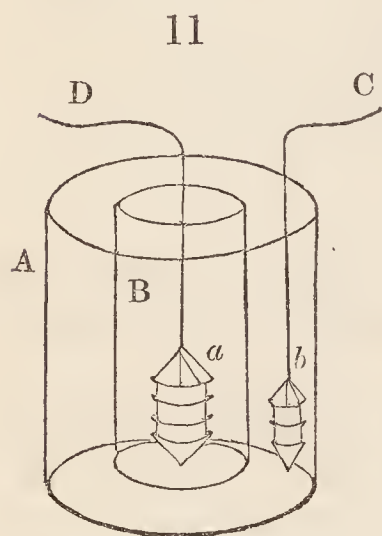
In experimenting with this apparatus, the top basin is filled with a solution of the substance which the operator is desirous of having formed into a crystalline state. A piece of pumice, or other porous stone, may be placed in the funnel on the middle shelf, and the electrodes of the battery adjusted to it, but not to touch each other. A weak solution of the sulphate, muriate, or nitrate of copper may be used in the basin above, and suffered to drip through the funnel on the stone below. Crystals of pure copper will soon be formed at the negative electrodes of the battery, and will spread themselves over, and even through the stone if it be sufficiently porous, in a most beautiful manner. Instead of the cupreous solutions, a solution of fluosilicic acid may be used, or a solution of silicate of potassa.

In making these experiments, it must be borne in mind, that the

drops from the upper compartment of the apparatus must *not* be suffered to fall on the electrodes, or where the crystallising process is going on; while, at the same time, the whole of the stone must be kept wet. The process succeeds best when carried on in a dark room.

It was while using an apparatus similar to that which I have described, with a muriated solution of silicate of potassa, that the celebrated insects were observed by Mr. Crosse on the stone on which the solution was dripping. It may perhaps be considered uncalled for, to hazard even an opinion on the apparent results of this and other experiments in which similar insects were said to have appeared; I shall, therefore, only state, that they have not occurred during any experiments that I have made with like apparatus and similar substances.

Mr. Crosse operates occasionally with a simple form of apparatus, which is represented in fig. 11, where A is an earthenware or glass vessel, containing a porous earthenware vessel B. C and D are two pieces of platinum wire, connected with a battery in such a manner as to form its positive and negative electrodes. One end of each wire is twisted round a piece of quartz; and the wires are placed one in each vessel, as shown by *a* and *b*. A solution of fluo-silicic acid is poured into *both* vessels, and the whole is placed in connection with the battery, and is then suffered to remain undisturbed in a darkened chamber for three or four months. At the expiration of this period, very perfect transparent crystals will be observed to have formed



on the piece of quartz forming the negative electrode of this arrangement.

In all electro-chemical decompositions I am acquainted with, when desirous of acting on a fluid with the electrodes of a voltaic arrangement, to ensure success, one condition, among others already enumerated, is necessary. This is, the electrodes must *not* be in contact. The nearer they are to each other, the more intense will be their action on the intervening fluid, and *vice versa*; but should they be made to *touch*, or be in what is usually termed metallic contact, *no* perceptible action will be observed. The electric current, no doubt, still traverses the wires, but the reason it does not act on the fluid as it passes, in such cases, is easily explained. Metals of all descriptions are much better conductors of electricity than fluid bodies. Some metals are worse conductors of electricity than others; but, when this occurs, these are much easier acted on than the metals possessing higher powers of conduction. Thus a given amount of electric action is sufficient to heat to redness fine platinum wire, but equal action would not heat an equal amount of copper wire of similar thickness. The reason is, that platinum is a worse conductor of electricity than copper.

From this and similar facts it is inferred, that the fluid or force

termed electrical, requires a *longer time* to pass through platinum than it does to traverse copper, or, in other words, it finds an easier or more open passage in the one metal than the other, and while passing through the platinum, sufficient time is taken to heat the metal to redness. These conclusions are arrived at inferentially; bearing them, however, in view, we are enabled to predicate the action of the electrodes when immersed in a fluid, and the cause of the necessity for their separation.

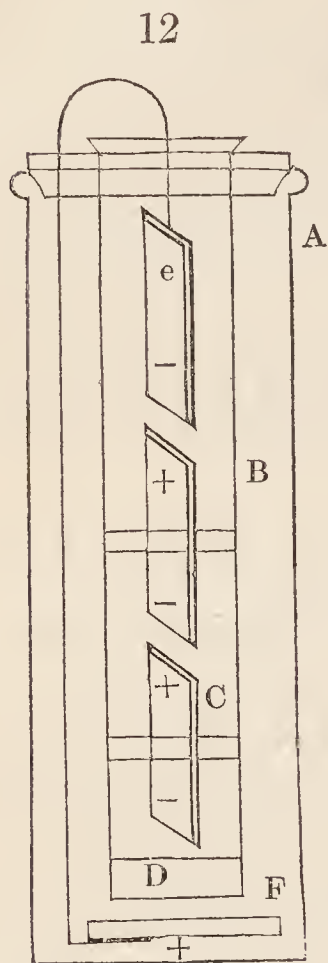
Fluids are worse conductors than any of the metals, and consequently would seem to require a still longer period, and greater exertion on the part of the electric force, while passing through them, than while the same force was passing through a metal. Hence ensues decomposition of the fluid. It will now be obvious, that should the electrodes be *in contact*, the electricity will *prefer* to pass through their metallic substance, and leave the fluid untouched. Still it must be borne in mind, they must not be placed too far apart in large quantities of fluids, otherwise the energy will be exhausted.

A very simple experiment may be made to show the manner in which electricity acts on metallic conductors, and on fluids. Take a few narrow slips of tin foil, and cause them to float in a continuous line in a trough containing acidulated water,—their ends must not be in contact. Now immerse the electrodes of a voltaic arrangement at the ends of the trough, and it will be found that one end of each slip will be oxidised, while the opposite end will evolve hydrogen. These phenomena are caused by the decomposition of the water. If, however, we cause the slips of tin foil to *touch* each other and the electrodes, no such action will take place, because water, in this latter instance, will not be decomposed, the electricity passing through the metallic conductor. In the first part of this illustration, it is worthy of remark that one end of each slip will be found *positive*, and the other *negative*, with regard to each other.

In connection with this subject, a beautiful variation of this experiment has been recently made by Dr. Brett. I have since repeated it with a slight alteration of some of the conditions, and have always met with similar results, as far as oxidisable metals have been experimented with. This experiment still farther illustrates the fact that deposition never takes place without an equivalent of oxidation.

Fig. 12. Take a tall glass jar, A, seven or eight inches high, and about four inches in diameter. Procure a gas glass or piece of tube, B, about two inches in diameter, and about one inch less in height than the jar. Fit a wooden top to the jar, in which is a round aperture of sufficient size to suspend the gas glass in such a position as to reach within an inch of the bottom of the interior of the jar. Make, also, a slit in the wooden top to admit a wire, as represented in the subjoined fig. 12. When the arrangement is thus far completed, suspend the slips of sheet copper, C, in the interior of the gas glass, taking care that the ends do not touch each other. It is most satisfactory if they are kept half an inch apart. This is best accomplished by fitting into

the interior of the glass, discs of cork in which notches are cut, to allow the fluid to communicate freely. The pieces of copper are fastened in their places by making a slit in each piece of cork, into which the slips of copper are inserted, projecting equally at both sides. The lower end of the gas glass must now be filled up with plaster of paris, D, so as to



make a porous bottom of about three quarters of an inch in thickness. Solder a third slip of copper, e, to one end of a piece of copper wire, and to the other end a slip of zinc, F. The wire must now be bent, as shown in the figure, and the gas glass be suspended in the interior of the jar, having the zinc in the exterior vessel, and the copper in the interior one. A saturated solution of sulphate of copper is now to be poured into the gas glass, and a solution of sulphate of soda or of common salt into the outside vessel. This arrangement being completed, and suffered to remain at rest for eight or ten days, a very singular appearance presents itself. The two suspended slips of copper apparently become polarised—one half of each being covered with crystals of pure copper, while the other half is oxidised, each slip presenting, as it were, an opposite pole to the other, standing in the relation of positive and negative, as indicated in the figure by the signs + and -.

This experiment is curious, and may be made with an indefinite number of suspended slips, and always with similar results. Slips of zinc, in a zincous solution, will also answer. To prove that it is not influenced by the density of the fluid in the interior vessel, I have had the top end of both vessels closed in such a manner as to reverse the whole apparatus without suffering the fluids to escape, but without, in the slightest degree, altering the effect of the experiment. When, however, the unconnected metallic slips are of platinum, no action takes place on either end.

Before concluding this portion of the subject, I would call attention to the circumstance, that in all the experiments hitherto detailed, no instance occurs of metallic deposition having taken place, except in absolute contiguity with the surface of a *metallic*, or, at least, a *metaliferous* substance.

Nothing has been yet adduced to show that chemical action takes place between the electrolyte and that portion of the body deposited on, as the deposit may be removed, and, on microscopic examination, no trace of action will be perceived to have taken place on its surface. In the former state of our chemical knowledge, when decomposition in one body took place on the approach of another, a definite alteration invariably took place in *both*, if not chemical union. A few years ago, this would have been deemed a chemical axiom, to which an apparently

anomalous exception or two might exist, but referable to no known law. Latterly, however, it has been found that there are substances capable of causing decompositions, and bringing about new unions between certain elementary and compound bodies, merely by virtue of their *presence*, and without *themselves* combining with either constituent.

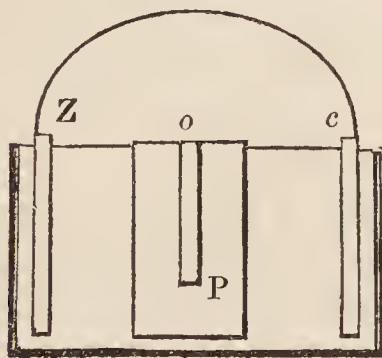
The observed cases in which this is assumed to take place, "have led to the invention of a particular term, *catalysis* (dissolution), to indicate the property in question, thus marking the distinction between what takes place here and in a process of *analysis*, where a new substance being presented to an existing combination, annuls it, by uniting itself with one of the constituents."*

The instance formerly adduced respecting spongy platinum, may be referable to this law, as also the fact, that a perfectly *clean* platinum plate will induce combination when placed in contact with mixtures of some of the gases; but the fact that metallic aggregation in the solid or crystalline form does not take place during electrolytic action, except in the presence and *mediate contact* of a metallic or metaliferous body, has not yet been referred to any law.

Whatever may be the precise action that takes place during any instance of catalysis, we are as yet unable to explain it; and at first sight it might appear, that the conditions requisite to induce metallic crystallisation, were referable to the same law, more especially when we find deposition taking place on a metallic body that is *not* in contact with the electromoter, and would appear to induce this action simply by its presence.

To ascertain these results as precisely as possible, the author made a series of continuous experiments† during a period of twelve months, and in *no instance* did metallic or other deposition appear, except in the presence and immediate contact of a metallic body, to form, as it were, a nucleus on which crystals of the nascent metal might aggregate.

13.



To test this by experiment, proceed as follows:—Take a small glass or earthenware box, fig. 13, as directed to be used in a former experiment. Let a plate of copper and one of zinc be soldered to each end of an arc of copper wire. Fasten these at each end of the box. Let plaster of paris be mixed with a saturated solution of sulphate of copper, and when set into a cubical piece that will go into the box, let it be

enveloped with gold beater's skin, or a piece of bladder. These precautions are necessary to prevent any of the crystals of the metallic deposit that takes place in the negative cell, from getting into the mass of plaster, and thereby interfering with the correctness of the results.

* Dr. Daubeny, Supplement to the Atomic Theory.

† The paper containing the results of these experiments was read at the first meeting of the Liverpool Polytechnic Society.

When this is carefully performed, place the plaster, P, in the centre of the box, or equidistant between the metallic electrodes, and let it be cemented in such a manner as to form a water-tight division of the box into a pair of separate cells. This done, bore a hole in the top of the division about half an inch in diameter, as in the section o. This is necessary to allow the mass to be kept *moist*, by pouring into it water or sulphate of copper, otherwise the electric current would not be suffered to pass.

Let a solution of sulphate of copper be placed in the cell containing the copper plate, and salt and water or sulphate of soda in the other. Voltaic action is now induced, and, according to the received theory, must pass through the porous division; but if this action be kept in continuity for an indefinite length of time, *no* metallic deposit will take place in the *interior* of the porous mass; on the contrary, a copious metallic deposition will take place on, and in *contact* with, the copper electrode in one of the cells. If, however, we place a slip of copper in the aperture, o, of the plaster division, a very different state of things will take place. Crystals of the pure metal will now be formed on that side of the centre slip that is opposed to the zinc, and an oxide of copper on the side opposed to the copper. The plaster division will now be permeated with crystals of copper proceeding from the point where it comes in contact with the metallic slip.

At the time these experiments were made, copper was the only metal made use of as a nucleus to insert in the aperture of the division, I have since, however, placed platinum in the position in which I formerly placed the copper slip, and have been unable to induce *any* action on its surface whatever. Palladium has also remained unaffected. In repeating this experiment, however, I found that the metallic deposit *never* takes place on one end of the intervening slips, without an equivalent amount of oxidation or other chemical action on the other; and in making the experiment by substituting slips of platinum or palladium for those of copper, they are not acted on at either end. I have been, therefore, led to refer the oxide of copper formerly found by me in the aperture, to that resulting from the oxidation of the metallic copper; and on repeating the former experiments, and narrowly inspecting the results, I find this to be the fact.

From the result of these and other experiments, I am led to the following conclusions:—First, That no metallic deposit will take place in cases of slow electrolytic decomposition, except in contact with a metallic or metalliferous nucleus;—and, Secondly, That it will not take place, unless induced by a *corresponding amount* of chemical action on another portion of the nucleus. Hence it is, that when a metal is placed in the circuit, which is incapable of being acted on by the fluid, such as platinum, no perceptible action takes place. It is, however, true, that copper may be deposited on platinum, or any other metal incapable of being oxidised, *per se*, but when this takes place, it must be *in connection* with another metal that is oxidisable. Should the platin-

um be placed in a solution of a chloride, then a deposit will take place on its surface.

It may not be unimportant to mention that in this, as well as in all similar experiments I am acquainted with, the metallic deposit is *an exact equivalent* of the metal oxidised at the opposite pole. Should a slip of charcoal be inserted, a slight deposit will be observed on the end *opposed* to the positive pole; but even in this case, an *equivalent* of oxidation will have taken place at the other end. This furnishes us with another analogy respecting the metallic nature of carbon.

In all electro-chemical experiments of long-continued action, and where metallic deposition is going on in one cell, and chemical decomposition taking place in the other, I have invariably found that the metallic crystals are deposited in vertical lines or ridges of crystallisation, which, as far as my own observation goes, are always perpendicular to the horizon; and, as if they obeyed the law of gravitation, they aggregate in largest amount at the lower end of the electrode. That they are influenced by this law when they assume such a disposition, seems highly probable; however, experiments are wanting to determine the correctness of these cursory observations.

I believe, in all recorded cases of electro-chemical decomposition, we are indebted for the action that ensues to the decomposition of water, in the first instance; but should the water be in the state of ice, decomposition will not take place, hence substances to be decomposed must be combined with water as a fluid. There is much difference of opinion as to the *modus operandi*; but the most probable one is, that the atoms of water intervening between the electrodes become polarised *throughout* their mass, and consequently the oxygen end of each atom is turned to the zinc or positive electrode, and the hydrogen end of each, to the copper or negative electrode, and the electricity that bound the two atoms of these gases together to form water, becomes liberated by their disruption. Some observers have recorded—among others Dr. Silliman—that they have *seen* with the naked eye, *the passage* of the gases from the one electrode to the other. This must have been a mistake: such *not* being the fact under any circumstances.

SECOND EDITION OF THE ORIGINAL PAPER

ON

WORKING IN METAL BY VOLTAIC ELECTRICITY,

REPRINTED FROM THE ONE PUBLISHED BY THE

LIVERPOOL POLYTECHNIC SOCIETY,

AND READ AT THE MEETING OF SEPTEMBER THE 12TH, 1839, NOTICE BEING
GIVEN MAY THE 8TH,

HENRY BOOTH, ESQ., PRESIDENT, IN THE CHAIR.

IN the paper that I have the honour to lay before the society, I do not profess to have brought forward a perfect invention. My only object is to point out a means by which, I hope, practical men may be enabled, to apply a great and universal principle of nature, to the useful and ornamental purposes of life. In this, I may be considered sanguine,—an error, I am aware, too often fallen into by those who, like myself, imagine they have discovered a useful application of an important principle; but however this may fall out, I shall lay an account of its results with specimens, successful and unsuccessful, before the members and the public,—previously stating, however, that all my first experiments were made on a small scale; a method of procedure attended with many advantages to the experimentalist himself, but having its disadvantage when laid before the public. In this first respect, perhaps, the chemical experimenter has an advantage over the mechanical one; as the success of his experiment, when tried on a small scale, doubly guarantees it, if conducted on a still larger scale—with mechanical results I believe in most instances it is the reverse. But when the chemist produces his microscopic proofs, the public are generally slow to believe that such minute appearances should warrant him in coming to any general conclusion.

In the latter part of September, 1837, I was induced to make some electro-chemical experiments, with single pairs of plates, consisting of small pieces of zinc and equal-sized pieces of copper, connected together with wires of the latter metal. It was intended that the action should be slow; the fluids in which the metallic electrodes were immersed, were in consequence separated by thin discs of plaster of

paris. In one cell thus formed was placed sulphate of copper in solution, in the other, a weak solution of common salt. I need scarcely add that the copper electrode was placed in the cupreous solution, the other being in that of the salt. I mention these experiments briefly,—not because they are *directly* connected with what I shall have to lay before the society, but because, by a portion of their results, I was induced to come to the conclusions I have done in the following paper. I was desirous that no action should take place on the wires by which the electrodes were held together; and to attain this object I varnished them with sealing-wax varnish:—but, in one instance, I dropt a portion on the copper electrode that was attached. I thought nothing of this circumstance at the moment, but put the experiment in action.

This operation was conducted in a glass vessel; I had consequently an opportunity of occasionally examining its progress from the exterior. After the lapse of a few days, metallic crystals had covered the copper electrode,—*with the exception of that portion* which had been spotted with the drops of varnish. I at once saw, that I had it in my power to guide the metallic deposition in any shape or form I chose, by a corresponding application of varnish, or other non-metallic substance.

I had been aware of what every one who uses a sustaining galvanic battery with sulphate of copper in solution must know,—that the copper plates acquire a coating of copper from the action of the battery; but I had never thought of applying it to a useful purpose, except to multiply the plates of a species of battery, which I did in 1836. My present attempt was with a piece of thin copper plate having about four inches of superficies, with an equal sized piece of zinc, connected as before by a piece of copper wire. I gave the copper a coating of soft cement, consisting of bees' wax, resin, and a red earth. It was compounded in the manner recommended by Dr. Faraday, in his work on chemical manipulation, but with a larger proportion of wax. The plate received its coating while hot. When it was cold, I scratched the initials of my name rudely on the plate, taking special care that the cement was quite removed from the scratches, that the copper might be thoroughly exposed. This was put in action in a cylindrical glass vessel about half filled with a saturated solution of sulphate of copper. I then took a common gas glass, similar to that used to envelope an argand burner, and filled one end of it with plaster of paris, to the depth of three-quarters of an inch. Into this I put water, adding a few crystals of sulphate of soda to excite action, the plaster of paris acting as a partition to separate the fluids, but, at the same time, being sufficiently porous to allow the electro-chemical action to permeate its substance.

I now bent the wire in such a manner that the zinc end of the arrangement should be in the saline solution, while the copper end, when in its place, should be in the cupreous solution. The gas glass, with the wire, was then placed in the vessel containing the sulphate of copper.

It was then suffered to remain at rest, when in a few hours I perceived that action had commenced, and that the portion of the copper rendered bare by the scratches had become gradually coated with pure bright deposited metal, whilst all the surrounding portions were not at all acted on. I now saw my former observations realized;—but whether the deposition so formed would retain its hold on the plate, and whether it would be of sufficient solidity or strength to bear working if applied to a useful purpose, became questions which I now determined to solve by experiment.

It also became a question—should I be successful in these two points—whether I should be able to produce lines sufficiently in relief to print from. This latter appeared to depend entirely on the nature of the cement or etching-ground I might use.

This I endeavoured to solve at once; and, I may state, it appeared at the time to be the main difficulty, as my impression then was, that little less than one-eighth of an inch of relief would be requisite to print from.

I now procured a piece of copper, and gave it a coating of a modification of the cement I have already mentioned, and having covered it to about one-eighth of an inch in thickness, I took a steel point and endeavoured to draw lines in the form of net-work, that should entirely penetrate the cement, and leave the surface of the copper exposed. But in this I experienced much difficulty, from the thickness I deemed it necessary to use; more especially when I came to draw the cross lines of the net-work. The cement being soft, the lines were pushed as it were into each other, and when it was made of harder texture, the intervening squares of the net-work chipped off the surface of the metallic plate. However, those that remained perfect I put in action as before.

In the progress of this experiment, I discovered that the solidity of the metallic deposition depended entirely on the weakness or intensity of the electro-chemical action, which I knew I had in my power to regulate at pleasure, by the thickness of the intervening wall of plaster of paris, and by the coarseness or fineness of the material. I made three similar experiments, altering the texture and thickness of the plaster each time, by which I ascertained that if the partitions were *thin* and *coarse*, the metallic deposition proceeded with great *rapidity*, but the crystals were friable and easily separated; on the other hand, if I made them thicker, and of a little finer material, the action was slower, but the metallic deposition was as solid and ductile as copper formed by the usual methods,—indeed, when the action was exceedingly slow, I have had a metallic deposition apparently much harder than common sheet copper, but more brittle.

There was one most important and, to me, discouraging circumstance attending these experiments, which was, that when I heated the plates to get off the covering of cement, the meshes of copper net-work occasionally *came off with it*. I at one time imagined this difficulty insu-

perable, as it appeared that I had cleared the cement entirely from the surface of the copper that I meant to have exposed; and I concluded that there must be a difference in the molecular arrangement of copper prepared by heat, and that prepared by voltaic action, which prevented their chemical combination. However, I determined, should this prove so, to turn it to account in another manner, which I shall relate in the second portion of the paper.

I now occupied myself for a considerable period in making experiments on this latter section of the subject.

In one of them I found, on examination, that a portion of the copper deposition, which I had been forming on the surface of a coin, adhered so strongly that I was quite unable to get it off,—indeed, a chemical combination had apparently taken place. This was only on one or two spots on the prominent parts of the coin. I immediately recollected that on the day I put the experiment in action, I had been using nitric acid for another purpose on the table I was operating on, and that in all probability the coin might have been laid down where a few drops of the acid had accidentally fallen. Bearing this in view, I took a piece of copper, coated it with cement, made a few scratches on its surface until the copper appeared, and immersed it for a short time in dilute nitric acid, until I perceived, by an elimination of nitrous gas, that the exposed portions were acted upon sufficiently to be slightly corroded. I washed the copper in water, and put it in action as before described. In forty-eight hours I examined it, and found the lines were entirely filled with copper. I applied heat, and then spirits of turpentine, to get off the cement, and, to my satisfaction, I found that the voltaic copper had completely combined itself with the sheet on which it was deposited.

I then gave a plate a coating of cement to a considerable thickness, and sent it to an engraver; but when it was returned, I found the lines were cleared out so as to be wedge-shaped, or somewhat in the form of a ∇ , leaving a hair line of the copper exposed at the bottom, and a broad space near the surface; and where the turn of the letters took place, the top edges of the lines were galled and rendered rugged by the action of the graver. This, of course, was an important objection, which I have since been able to remedy in some degree, by an alteration in the shape of the graver, which should be made of a shape more resembling a narrow parallelogram than those in common use,—some engravers have many of their tools so made. I did not put this plate in action, as I saw that the lines, when in relief, would have been broad at the top and narrow at the bottom. I took another plate, gave it a coating of the wax, and had it written on with a mere point. I deposited copper on the lines, and afterwards had it printed from.*

I now considered part of the difficulties removed; the principal one yet remaining was to find a cement or etching-ground, the texture

* This plate was shown to friends, and also specimens of printing from it, in 1833.

of which should be capable of being cut to the required depth, without raising what is technically termed *a burr*, and, at the same time, of sufficient toughness to adhere to the plate, when reduced to a small isolated point, which would necessarily occur in the operation which wood engravers term cross-hatching.

I have since learned, from practical engravers, that much less relief is necessary to print from than I had deemed indispensable, and that on becoming more familiar with the cutting of the wax-cement, they would be enabled to engrave in it with great facility and precision.

I tried a number of experiments with different combinations of wax, resins, varnishes, earths, and metallic oxides, all with more or less success. One combination that exceeded all others in its texture, was principally composed of bees' wax, resin, and white lead. This had nearly every requisite, so that I was enabled to polish the surface of the plate with it until it was nearly as smooth as a plate of glass. With this compound I had two plates, five inches by seven, coated over, and portions of maps cut on the cement, which I had intended should have been printed off. I applied the same process to these as to the others, immersing them into dilute nitric acid before putting them in action; indeed, I suffered them to remain about ten minutes in the solution. I then put them into the voltaic arrangement. The action proceeded slowly and perfectly for a few days, when I removed them. I applied heat, as usual, to remove the cement, but *all* came away, as in a former instance, the voltaic copper peeling off the plate with the greatest facility. I was much puzzled at this unexpected result; but, on cleaning the plate, I discovered a delicate trace of *lead*, exactly corresponding to the lines drawn on the cement previous to the immersion in the dilute acid. The cause of this failure was at once obvious: the carbonate of lead I had used to compound the etching-ground had been decomposed by the dilute nitric acid, and the metallic lead thus reduced had deposited itself on the exposed portions of the copper-plates, preventing the voltaic copper from chemically combining with the sheet copper. I was now with regret obliged to give up this compound, and to adopt another, consisting of bees' wax, common resin, and a small portion of plaster of paris. This seems to answer the purpose tolerably, though I have no doubt, by an extended practice, a better may still be obtained by a person practically acquainted with the etching-grounds in use among engravers.

I now proceed to the second, and I believe the most satisfactory portion of the subject. Although I have placed these experiments last, some of them were made at the same time with the others already described, and some of them before; but, to render the subject more intelligible, I have placed them thus.

The members of the society will recollect, that on the first evening it met, I read a paper on the "production of metallic veins in the crust of the earth," and that among other specimens of cupreous crystallisation which I produced on that occasion, I exhibited three

coins; one wholly covered with metallic crystals, the other on one side only. It was used under the following circumstances. When about to make the experiment, I had not a slip of copper at hand to form the negative end of my arrangement, and, as a good substitute, I took a penny and fastened it to one end of the wire, and put it, in connection with a piece of zinc, in the apparatus already described.

Voltaic action took place, and the copper coin became covered with a deposition of copper in a crystalline form. But, when about to make another experiment, and being desirous of using the piece of wire used in the first instance, I pulled it off the coin to which it was attached. In doing this, a piece of the deposited copper came off with it; on examining the under portion of which, I found it contained an exact mould of a part of the head and letters of the coin as smooth and sharp, in every respect, as the original on which it was deposited. I was much struck with this at the time; but, on examination, the deposited metal was very brittle. This, and the fact that it would require a metallic nucleus to aggregate on, made me apprehensive that its future usefulness would be materially abridged; but it was reserved for future experiment, and in consequence laid aside for a time, until my attention was recalled to the subject in a subsequent experiment already detailed, by the drops of varnish on a slip of copper. Finding in that instance that the deposit would take the direction of any non-conducting material, and be, as it were, guided by it, I was induced to give the previous branch of the subject a second trial, because I had in the first instance supposed that the deposition would only take place continuously, and not on isolated specks of a metallic surface, as I now found it would; but the principal inducement to investigate the subject was the fact of finding that deposited copper had much more tenacity than I at first imagined.

Being aware of the apparent natural law which limits metallic deposition by voltaic electricity, excepting in the presence of a metallic body, I perceived that the uses of the process would in consequence be extremely limited, except in the multiplication of already engraved plates, as, whatever ornament it might produce, it would only be done by adhering to the condition of a metallic mould.

I accordingly determined to make an experiment on a very prominent copper medal. It was placed in a voltaic circuit, as already described, and deposited a surface of copper on one of its sides, to about the thickness of a shilling. I then proceeded to get the deposition off. In this I experienced some difficulty, but ultimately succeeded. On examination with a lens, every line was as perfect as the coin was from which it was taken. I was then induced to use the same piece again, and let it remain a much longer time in action, that I might have a thicker and more substantial mould, in order to test fairly the strength of the metal. It was accordingly put it again in action, and let remain until it had acquired a much thicker coating of the metallic deposition; but on attempting to remove it from the medal, I found I was unable. It had, apparently, completely adhered to it.

I had often practised, with some degree of success, a method of preventing the oxidation of polished steel, by slightly heating it until it would melt fine bees' wax; it was then wiped, apparently completely off, but the pores or surface of the metal became impregnated with the wax.

I thought of this method, and applied it to a copper coin.

I first heated it, applied wax, and then wiped it so completely off, that the sharpness of the coin was not at all interfered with. I proceeded as before, and deposited a thick coating of copper on its surface. Being desirous to take it off, I applied the heat of a spirit lamp to the back, when a sharp crackling noise took place, and I had the satisfaction of perceiving that the coin was completely loosened. In short, I had a most complete and perfect copper mould of one side of a half-penny.

I have since taken some impressions from the mould thus taken, and, by adopting the above method with the wax, they are separated with the greatest ease.

By this experiment, it would appear, that the wax impregnates the surface of the metal to an inconsiderable depth, and prevents a chemical adhesion from taking place on the two surfaces; and I can only account for the crackling noise, on separation, by supposing it probable that the molecular arrangement of the voltaic metal is different from that subjected to percussion, and this difference causes an unequal degree of expansibility on the application of heat.

I became now of opinion, that this latter method might be applied to engraving much better than the method described in the first portion of this paper. Having found in a former experiment that copper in a voltaic circuit deposited itself on lead with as much rapidity as on copper, I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a common screw press. From the softness of the lead, I had a complete and sharp mould of both sides of the coin, without it sustaining injury. I then took a piece of copper wire, soldered the lead to one end, and a piece of zinc to the other, and put them into the voltaic arrangement I have already described. I did *not*, in this instance, *wax* the mould, as I felt assured that the deposited copper would easily separate from the lead by the application of heat, from the different expansibility of the two metals.

In this result I was not disappointed. When the heat of a spirit-lamp was applied for a few seconds to the lead, the copper impression came easily off. So complete do I think this latter portion of the subject, that I have no hesitation in asserting that fac-similies of any coin or medal, no matter of what size, may be readily taken, and as sharp as the original. To test further the capabilities of this method, I took a piece of lead plate, and stamped some letters on its surface to a depth sufficient to print from, when in relief. I deposited copper on it, and found it came easily off, the letters being in relief.

Finding from this experiment that the extreme softness of lead allowed it to be impressed on by type metal, I caused a small portion of

ornamental letter-press to be set up in type, and placing it on a planed piece of sheet lead, it was subjected to the action of a screw press.

After considerable pressure, it was found that a perfectly sharp mould of the whole had been obtained in the lead. A wire was now soldered to it, and it was placed in an apparatus, similar in principle, but larger than the one already described. At the end of eight days from this time, copper was deposited to one-eighth of an inch in thickness; it was then removed from the apparatus and the rough edges of the deposited copper being filed off, it was subjected to heat, when the two metals began to loosen. The separation was completed by inserting a piece of wedge-shaped wood between them.

I had now the satisfaction of perceiving that I had by these means obtained a most perfect specimen of stereotyping in copper, which had only to be mounted on a wooden block to be ready to print from.*

From the successful issue of this experiment, which was mainly due to the susceptibility of the lead, I was induced to attempt to copy a wood engraving by a similar method, provided the wood would bear the requisite pressure. Knowing that wood engravings are executed on the *end* of the block, I had better hopes of succeeding, the wood being less likely to sustain injury.

I accordingly procured a small wood block and placed its engraved surface in contact with a piece of sheet lead made very clean, and subjected it to pressure as in the former instance. I had now, as before, the gratification of perceiving that a perfect mould of the little block had been obtained, and no injury done to the original. Several wood engravings and copper plates were subjected to similar treatment, and are now in process of being deposited on in the apparatus before me.†

I now come to the third and concluding portion of the experiments on this subject. The object being to deposit a metallic surface on a model of clay, wood, or other *non-metallic* body,—as, otherwise, I imagined the application of this principle would be extremely limited. Many experiments were made to attain this result, which I shall not detail, but content myself with describing those which were ultimately most successful.

I procured two models of an ornament, one made of clay, and the other of plaster of paris, soaked them for some time in linseed oil, took them out, and suffered them to dry—first getting the oil clean off the surface. When dry, I gave them a thin coat of mastic varnish. When the varnish was nearly dry, *but not thoroughly so*, I sprinkled some bronze powder on that portion I wished to make a mould of. This powder is principally composed of mercury and sulphur, or it may be chemically termed a sulphuret of mercury. There is a sort that acts much better,

* Specimens of printing from this block was subsequently distributed to members of the society.

† Several specimens of printing from copper fac-similies of these wood engravings subsequently appeared in the *Liverpool Mercury*.

in which is a portion of gold. I had, however, a complete metalliferous coating on the surface of the model, by which I was enabled to deposit a surface of copper on it, by the voltaic method I have already described. I have also gilt the surface of a clay model with gold leaf, and have been successful in depositing copper on its surface. There is likewise another, and, as I trust it will prove, a simpler method of attaining this object; but as I have not yet sufficiently tested it by experiment, I shall take another opportunity of describing it.*

[At the close of the paper, several specimens of coins, medals, and copper plates, some of them in the act of formation by the voltaic process, were exhibited to the members.]

* On subsequent experiment, the process alluded to was perfectly successful and is described in page 39.

PROCESSES.

TO OBTAIN ANY NUMBER OF COPIES OF AN ALREADY-ENGRAVED COPPER-PLATE.

TAKE a copper-plate, engraved in the common manner—the lines being in *intaglio*. Procure an equal-sized piece of sheet-lead; lay it on the engraved side of the plate, and put both under a *very powerful* press; when taken out, the lead will have all the lines of the copper plate in relief.

A wood engraving may be operated on in like manner;—as pressing into lead will not injure it.

A wire must now be soldered to the lead, and the whole put into the apparatus,—when a copper-plate, being an exact fac-simile of the original, will be formed; its thickness entirely depending on the length of time it is suffered to remain.

In this process care must be taken that the lead is clean and bright, as it comes from the roller in the milling-process, and consequently free from the oxidation which it acquires on exposure to the atmosphere. It should be put in action as soon as possible after being taken out of the press.

I have been able to copy some small copper-plates, by obtaining impressions from them in lead; but have not had the convenience of a powerful press to enable me to copy large engravings. By adopting lead, where convenient, the process of producing a fac-simile is abridged one half, but it is important that the surface of the lead be perfectly *clean*, and this is best attained by planing it in the manner that joiners plane wood.

For copying small wood engravings, this method is peculiarly applicable; and I am induced to think that it will also answer well for the plates used in the manufacture of pottery, because very large ones are never required for this kind of printing, and the lines in the engravings are much deeper than is commonly the case with engravings for book-work—being required to hold a much greater quantity of ink. I have in consequence obtained impressions of them in lead with much more facility.

Where, however, an impression in lead from a given plate is not ob-

tainable, the best method is to procure one in copper by the voltaic process, and cause this to be the matrix on which to deposit the required fac-simile of the original.

The rules apply in this case that are applicable in others. It may, perhaps, be necessary to state that the original plate should be washed in water saturated with an alkali—potash does very well. It should never be washed with dilute acid, as in that case there is much danger of the deposited copper adhering to the original, more especially should the plate be a new one.

These precautions to prevent adhesion are more necessary, where it is requisite to deposit on voltaic copper, than on that prepared by the common method.

Should it at any time be necessary to use an acid to clean copper to be deposited on, the metal ought afterwards to be washed with an alkali.

I have been most successful in getting an easy separation of the metals when I have previously given the original a coating, while hot, of a mixture of bees' wax and turpentine, and then wiping the *whole* apparently off. It would appear that by this treatment, enough of the mixture still adheres to the surface to prevent chemical adhesion from taking place. In no case, however, must this be wiped, as if with the intention of *leaving* any portion of it, however small, as it would materially interfere with the uniformity of the deposition, and with the sharpness of the resulting plate.

When the copper is deposited to the required thickness, the next operation is to separate the original plate from the copy. First let the rough edges of the deposit be filed off, bringing it flush with the edge of the plate deposited on; when thus cleared, let the plates be subjected to the heat of a charcoal or coke fire, or if the plate is a small one, to the heat of a lamp. The plate *deposited on* should be placed in contact with the heat. When heated to about 300° Fahrenheit, pour suddenly some cold water on the *deposited* plate, and strike the other with some metallic substance to cause it to vibrate. On examination it will be found the plates are beginning to separate at the edges. Now insert a small wedge of wood, after which a few taps with a hammer completes the operation.

The plate thus obtained may be deposited on in its turn, being treated in all respects like the original.

TO PRODUCE A VOLTAIC IMPRESSION FROM A PLASTER, WOODEN, OR CLAY
MODEL OR MOULD.

THE first part of this process is partially described in the preceding paper; in addition to which I may state, that when the plaster or clay ornament is gilt with gold leaf, or bronzed, a copper wire should be attached to it, by running it through from the back until the point

touches the front metallised surface. The other end must then be so attached as to connect it with the zinc, in all respects similar to any of the foregoing methods.

In addition to these I have since adopted a method of attaining this object, which has been perfectly successful—having always considered the former to be at best but clumsy expedients, since in all cases the sharpness of the originals was injured. But the present method is purely chemical, and also presents an elegant experiment.

I had attempted to metallise surfaces to be deposited on, by plum-bago or black lead, which is chemically termed a carburet of iron; but this possessed some of the faults common to the others, and in many instances the deposition went on partially.

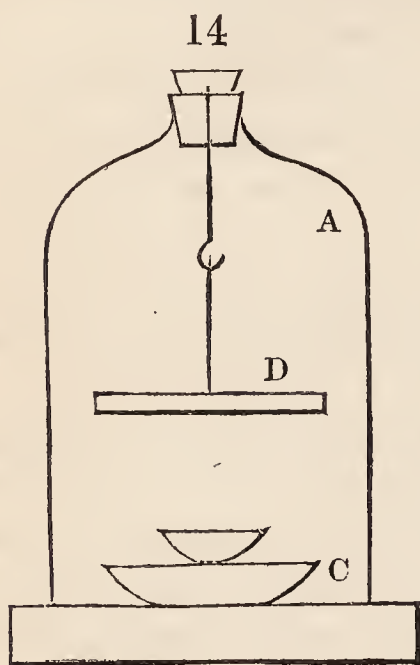
The process I am about to describe completely obviates all these objections, and leaves the surface of the material to be acted on as sharp as it was previous to the operation.

To obtain a copper mould or cast from a piece of wood, plaster, or clay, or, indeed, any non-metallic material, proceed as follows:—Suppose it to be an engraved wooden block which you are desirous of metallising, in order that copper may be deposited on its surface; this example holding good for any other material. The first operation is to take strong alcohol or spirits of turpentine in a glass vessel, and add to it a piece of phosphorus (a common phial corked will answer the purpose); the vessel must now be placed in hot water for a few minutes, and occasionally shaken. By this means the alcohol will take up about a 300th of its bulk of phosphorus, and we thus obtain a solution of phosphorus. Next procure a *weak* solution of nitrate of silver, place it in a flat dish or saucer; the engraved face of the block must now be dipped in this solution, and let remain for a few seconds, to allow capillary action to draw the nitrate of silver into the wood.

This operation being performed, a small portion of the solution of phosphorus must be poured in a capsule or watch-glass, and this be placed on a sand-bath, that it may gradually evaporate. The block must now be held with its surface over the vapour, and *an immediate change* takes place; the nitrate of silver becomes deoxidized, and gives place to *metallic* silver, which allows the voltaic deposit to go on with as much rapidity and certainty as the purest silver or copper.

The whole process may be performed in a few minutes, and with absolute certainty of success. The interior or exterior surface of a plaster or clay mould of a statue, no matter what size, may be thus metallised with equal facility. For the process of vaporising, when the material to be acted on is not very large, fasten it to the top of a bell-glass receiver with a bit of pitch or cement, and place it thus over the capsule on the sand-bath; the phosphoric vapour is by this means equally diffused, and not dissipated. A solution of phosphorus in sulphuric ether also answers; and a solution of chloride of gold may be used. I am inclined to think this process, independent of its uses in galvanic precipitation, may be applicable to other branches of art. I

would recommend those desirous of testing its effects to take a small and sharp plaster of paris medallion; dip its *surface* in a weak solution of nitrate of silver, and *take it out immediately*, fasten it to the bottom of a glass tumbler, and at the same time have a little hot sand ready in a dish; lay a porcelain capsule, containing a few drops of the phosphoric solution on it; place the mouth of the tumbler, or receiver, as

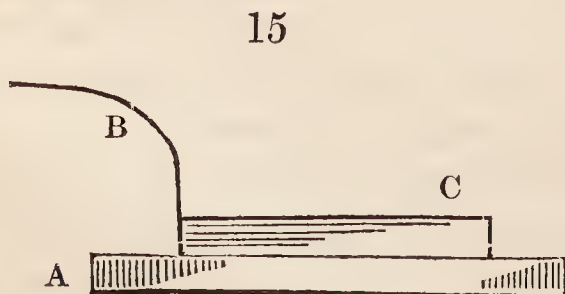


in the figure, over all, and the medallion will be observed almost instantly to change colour. The operation is now completed. Figure 14 explains the process on a large scale. A, a bell-glass receiver resting on a block of wood. C, the sand-bath containing the capsule with the phosphoric solution. D, the medallion or surface to be metallised, suspended over the capsule by a hooked wire from the top. The thread may be fastened to the back of the medallion with a piece of sealing wax or cement. I found other solutions that perform this decomposing operation; but the best and least expensive is to use spirits of turpentine to dissolve the phosphorus, as it takes up a greater

proportion of that substance, than alcohol. Sulphurous acid gas may also be used.

This operation is very successful when applied to wood. By its agency we also possess the means of metallising, in the first instance, and casting, in the second, a statue from its clay or plaster mould.

Suppose the surface of a substance metallised, it becomes of some importance to know how to apply the wire to it, in order that it may be properly fixed in the galvanic apparatus. Should it be a piece of wood we are desirous of depositing on, it will be sufficient to bore a small hole in either of the sides, into which the wire may be fastened, after which a small camel hair brush must be dipped into the nitrate of silver, and a line drawn with it, leading *from* the wire *to* the metallised surface; when dry, another brush must be dipped into the phosphoric solution, and with it the same line retraced. The de-



posited metal will thus be led from the wire to the metallised surface. If a piece of plaster of paris be required to be deposited on, the connecting wire must be fastened to a piece of *wood*, to serve as a support on which to place the plaster cast, and the wire must be bent in such a manner as to *touch* the metallised

surface, as shown in the affixed section, figure 15, where, A B represent the wooden support with the wire attached, and C the plaster or clay model to be deposited on.

TO PROCURE FAC-SIMILES OF MEDALS, &c.

THIS may be done by different methods; the first, by depositing a *mould* of the voltaic metal on the face of the medal, then depositing copper, by a subsequent operation, in the mould so formed. The voltaic metal will require the application of wax, but in all cases where this is applied, it must be wiped off.

But the more ready way is to take two pieces of milled *sheet* lead, cast lead not being equally soft, having surfaces perfectly clean and free from indentation. Put the medal between the two pieces of lead, and subject the whole to the action of a press. A complete mould of both sides is thus formed in the lead, showing the most delicate lines perfectly, but in reverse. Twenty—or even a hundred—of these moulds may be so formed on one sheet of lead, and may be deposited on by the voltaic process with equal or greater facility than a single mould; as, the more extensive the apparatus, the more regularly and expeditiously the operation proceeds. Those portions of the surface of the lead, where the moulds do not occur, may be varnished, to neutralise the voltaic action; or, a whole sheet of copper being deposited, the voltaic medals may afterwards be cut out, or left in a sheet at pleasure, the blank portions being filled up with engraving descriptive of the medal or coin.

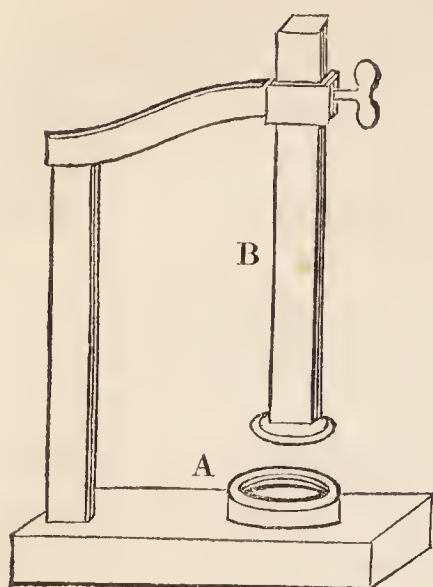
A piece of wire must now be soldered neatly to the back of the leaden plate; it is then ready to be put into the apparatus. In some cases, soldering may be dispensed with, by boring a hole in the piece of lead, and inserting a wire. Should the medals to be copied be large, it will be requisite to have a cell turned in the lead to fit them into, just deep enough to allow about half their thickness to project above the surface of the lead to sustain the pressure; by this means we obtain a thicker impression than we otherwise should, and having a rim also, the medal or cameo to be copied is less subject to injury.

Another method may be adopted, by using what is termed Newton's fusible metal. This being a compound of three parts of tin, five of lead, and eight of bismuth. This may be melted in *boiling water*. This metal is capable of giving exceedingly sharp impressions. A still better would, however, be what is termed *Smith's solder for tin*, as it is not so liable to crystallize in cooling as the fusible metal. This is composed of one part each of lead and tin, and two parts of bismuth.

Apparatus employed.—The machines used in striking these medalions are various; the common screw press will answer this purpose tolerably well, though the gradual pressure which it gives is not to be compared to the effect of a sudden blow. Another method, and one which has been tried with perfect success, is to pour the metal on a piece of leather or paper; when it begins to cool, so as to be scarcely fluid, place the copy upon it, rest a flat-ended stick upon it, and give the top end of the stick a slight blow. The figure 16 shows a contriv-

ance for the same purpose. It represents a stand with an upright

16



bent arm, through a square hole in the top of which passes a square solid rod B. The metal is poured into the metal cup^{*} A, and when at the right degree of heat, the rod B, which has the medal fastened with a bit of wax to the lower end, is to be let down, and a sudden blow to be given to the top, when the force will cause the impression to be a good one, if carefully performed. After the medallions are deposited on, the backs of them may be turned flat, and the edges turned round in a lathe adapted to the purpose.*

TO DEPOSIT SOLID VOLTAIC PLATES, HAVING THE LINES IN RELIEF.

TAKE a plate of copper, lead, silver, type-metal, or any metallic substance, of the required size, and engrave in it to the depth requisite to print from when in relief: then solder to it a piece of copper wire.

Contrary to ordinary engraving, the lines must be *flat* at the bottom, and as nearly as possible of the same depth. When so engraved, heat the copper plate, and apply wax to the plate, in the manner already mentioned, to prevent the voltaic copper from adhering.

The plate must now receive a couple of coats of varnish on the back and edges, the varnish consisting of a preparation of shell-lac and alcohol, or of melted bees' wax. Care must be taken to keep the engraved surface of the plate clean.

It is now ready to be placed in the apparatus to be deposited on.

Should the plate be lead, or, what is still better, type-metal, the wax does not require to be applied, in the first instance to the front of the plate, as, when it is deposited on to the given thickness, the application of heat is alone sufficient to loosen the plates.

I do not anticipate that this method of procedure will ever prove very generally useful, although, in some instances, I can believe it may be advantageous to adopt it. Where strong outlines are required to print along with matter set up in type, it may be economical, as in geometrical figures, or those of crystals, &c.

Lines in wood engravings are *left standing*, all the surrounding matter having to be *cut away*, necessarily entailing much labour; but, by this method, all the lines that are required ultimately to be in relief, would have to be sunk in the plate of soft metal, a process that would require little more time to perform than drawing them.

* Magazine of Science,—a useful publication.

The tools to cut the metal could be formed with flat faces, and with a guage to indicate when they had arrived at the proper depth.

The engravings necessary for a whole work on geometry, for instance, or where diagrams are requisite, might be so cut in one single sheet or plate of type-metal, the letters or figures of reference being punched in, and the whole deposited on *at once*.

When deposited to a sufficient thickness, the voltaic plate, having the lines and letters in relief, might then be separated into the requisite number of pieces, and each mounted, with roman cement moistened with drying oil, on small blocks of wood, to bring them up to the level of type.

Should any of the lines rise above the others, they may be smoothed down by rubbing the face of the block on a *smooth* flag, with a little water, and the lines, if necessary, could now be retouched with the graver. To a small extent I have seen this plan adopted with perfect success.

ANOTHER METHOD.—I have occasionally tried another method, which, in the hands of a practical engraver, might be useful. Take a copper or brass plate, and give it a coating of fine plaster of paris, or other substance. When dry, let the surface of the plaster be rubbed down level, until there is as much thickness left as the lines are required to be in relief to print from. It must now be smoked with the flame of a lamp, and the lines drawn in the surface thus prepared. The brass or copper plate will prevent the point used from going deeper than is necessary, consequently the lines will be all of the same depth. When the engraving is completed, let the plaster surface be metalised with the nitrate of silver and the phosphoric solution, as already described. This done, a plate of solid copper may be deposited on its surface, all the lines being perfectly level.

TO RAISE LINES IN RELIEF ON A PLATE OF COPPER OR OTHER METAL.

TAKE a plate of copper, or any other metal. It is not essential that it should be highly polished.

Have a piece of copper wire neatly soldered to the back part of it, and then give it a coating with a camel hair brush of either of the cements already mentioned. This is best done by heating the plate as well as the wax; or, to level the wax after it has had a coat, hold the back part of the plate over a charcoal fire or a spirit-lamp,—taking care to have it level.

Then write, or draw the design, on the wax or cement with a point. The wax must now be cut through with a graver or steel point, taking special care that the copper *is exposed in every line*.

It must now be immersed in dilute nitric acid—say three parts water to one acid. It will at once be seen whether it is strong enough, by the

green colour of the solution, and the bubbles of nitrous gas eliminated. Let it remain long enough to allow the exposed lines on the plate to be *slightly* corroded, that the wax, which gets into the pores of the copper during the heating process, may be thoroughly got rid of. Practice will determine this better than any rules.

The plate is now ready to be placed in the voltaic apparatus. After the voltaic copper has been deposited on the lines engraved in the wax, the surface of the formation will be found to be more or less rough, according to the quickness of the action. To remedy this, rub the surface with a piece of smooth flint or pumice-stone with water. Then heat the plate, and wash off the wax groundwork with spirits of turpentine and a brush.

A card plate, having the lines in relief on this plan, was procured by me more than two years ago; a few copies were printed from it, but they were coarse.

TO FORM PLAIN COPPER PLATES.

PLATES, having the surfaces burnished for the purposes of engraving may be made by the depositing process by using a highly polished or burnished copper or silver surface to deposit on, the deposited or voltaic plate having a surface *as bright* as the one on which it was deposited. Engravers prefer the plates so formed.

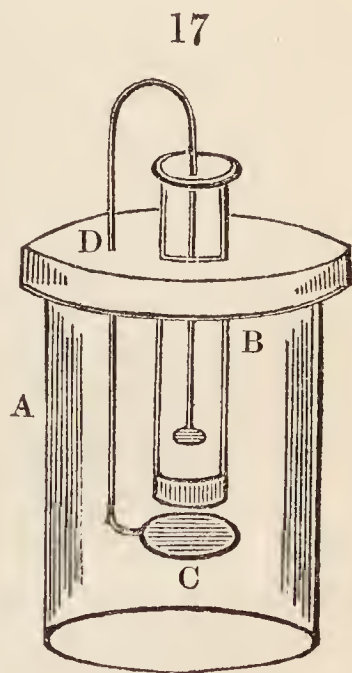
THE

APPARATUS AND ITS MANAGEMENT.

WHEN I first made the principles of this discovery public, I stated my expectation of seeing it improved, and carried into practice in the workshop of the artizan, who might adopt any of its various applications to aid him in the practice of his trade, providing it were found advantageous in a pecuniary sense. That this will be the case ultimately I now entertain little doubt, a very short period having elapsed since its publication.

The first kind of apparatus used by me to copy plates on copper, or coins, is shown in fig 17. A, represents a glass or tumbler containing the copper solution. B, a gas-glass, having one end closed with brown paper or plaster of paris, and containing the saline solution. C, the plate or medal required to be deposited on, having a wire attached, to the other end of which is a piece of zinc,—the wire being bent into the requisite form. D, a wooden cover, into which the gas glass fits. Should one of those glasses be used that has a projecting rim, it may then be very readily suspended from the top, care being taken that it is about an inch or more from the bottom of the exterior vessel. This apparatus is adapted for experiments on a small scale; or to take a fac-simile of a single medal.

Previous to having used an apparatus of this description, for the present purposes, I had been familiar with a similar arrangement from the period that Mr. Daniell published an account of his constant battery. At that time I had one constructed according to that gentleman's plan, when I found the ox-gullets recommended by him to be exceedingly troublesome. I endeavoured to improve on it, and in consequence made a battery with six tumblers, and as many gas-glasses, at the bottom of each of which was fastened a piece of bladder



to separate the fluids. The zinc and copper plates of each were placed exactly as in the figure. I need scarcely say that this was by no means a powerful arrangement, yet with it I was enabled to perform sundry experiments, and to a small extent decompose water.

Subsequently, when Dr. Bird, at the Liverpool meeting of the British Association, in 1837, gave an account of an experiment he had made, the apparatus described by him was also somewhat similar to the figure.

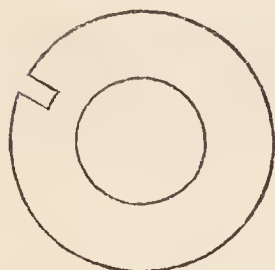
To use this apparatus for the purpose of producing a voltaic impression from a coin or other small work of art, proceed as follows:—

18



Fasten the coin or medal to a piece of wire by soldering, or any other method which will keep the wire in *contact* with the subject to be deposited on. To prevent injury, it may be fastened

19

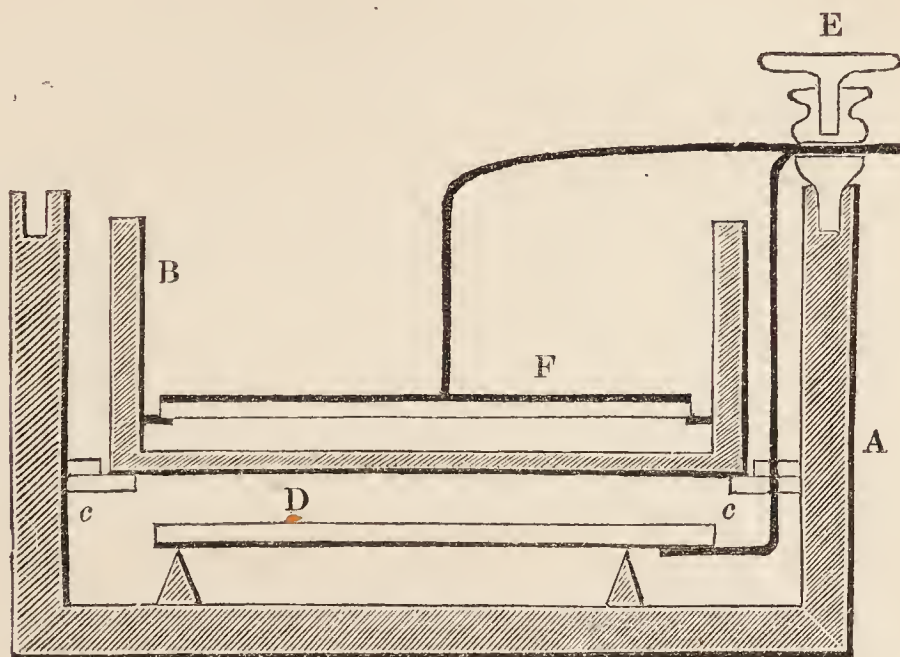


to the wire with Newton's fusible metal, elsewhere described. Then solder a piece of zinc to the other end of it. Fig. 18. shows the wire with the two plates attached. This wire must now be bent in such a manner as to allow the zinc end to be above the copper one, and, at the same time, to be exactly opposite each other, as shown in figure 17. A slit aperture must be made in the wooden cover, as in fig. 19, to allow the wire to get to its place.

To close the end of the gas-glass, I formerly used plaster of paris, I now find that a very thick sort of brown paper answers very well. It is fastened on the glass by giving the edge a coat of cement with a small brush; the glass must be slightly heated, and while in that state the disc of brown paper may be fastened to it, which performs the same functions as the plaster, and has the additional advantage of very materially quickening the process. The outside vessel must now be nearly filled with a saturated solution of sulphate of copper, and it is advisable *to have a quantity of the crystals of that salt always undissolved in the fluid*. The gas-glass containing the zinc must also be nearly filled with water, in which a *few* crystals of sulphate of soda have been previously dissolved. The sulphate of soda is put in merely to excite action in the *first instance*. It not being afterwards necessary to add any portion of this salt, sufficient action being kept up by the sulphuric acid set free in the other cell, which, passing into this, combines with the oxide of zinc to form a sulphate of zinc, which may be afterwards removed and used for commercial purposes, provided the operation be performed on a large scale.

My next form of apparatus was exactly the same in principle as the one above described, but much larger. Figure 20 represents a section of it. A, a box, which may be made of either earthen-ware, wood, or glass, and any size which may be found convenient. B, a box of either

20



of the above materials, but having a thick brown paper, or thin *un-glazed* earthen-ware bottom, the sides being impermeable by a fluid. This must be made to go into the interior of A, and to rest on the rim *c c*, running round the interior of the larger box, taking care, at the same time, that there shall be an inch or more of free space, according to the size of the apparatus, between the exterior of the small box and the interior sides of the larger one. The rim *c c* must be perforated with small holes, and it must be made to support a supply of crystals of sulphate of copper. D, the plate to be deposited on, to which a wire is attached leading to the binding screw E, fastened to the top of the exterior box. F, the plate of zinc to which a wire is attached, also leading to the binding screw. The connection between the two plates is thus rendered perfect, and either may be conveniently removed without disturbing the others. A few projecting studs of wire in the sides of the interior box, is also found useful, to keep the zinc plate perfectly parallel, and to prevent it from touching the porous bottom. They must be varnished, to prevent local action, or if the vessel be made of earthen-ware, they may be attached to the sides in the first instance, and be of the same material. The plate to be deposited on may be kept in its place by similar studs, or moveable pieces of earthen-ware, as shown in the section. This apparatus may be made of any convenient size, and either round, oblong, or square. The ends of the wires must be brightened before they are pressed together, by the binding screw. When all is arranged, as shown in the figure, pour into the outer box a saturated solution of sulphate of copper until it is nearly full, and then pour into the interior one containing the zinc, a weak solution of sulphate of zinc. At this stage of the proceeding, let a

quantity of the undissolved crystals of sulphate of copper be placed on the perforated rim. Should this be neglected for any length of time, a brown powder will be deposited on the plate instead of the pure metal. This substance is protoxide of copper, and always will be precipitated when the liquid is not kept thoroughly saturated with sulphate of copper. In summer the deposition goes on much quicker than when the weather is cold, as heat very much facilitates chemical decomposition; but artificial heat may be applied with advantage, and a given thickness of deposition be acquired in half the time, or according to the increment of heat. At a common temperature, however, of 65° Fahrenheit, by good management, a thickness of deposited metal, equal to $\frac{1}{8}$ of an inch, may be obtained in four days, such thickness, however, seldom being required. When this description of apparatus is used, the zinc should *not* be amalgamated, and it should *invariably* be of *equal* size to the plate or substance to be deposited on. If the side of the zinc plate that is *not* opposed to the copper one be varnished with shell-lac, or any other sort of varnish, so much the better, as it prevents local action taking place between the two sides. This must *always* be done to the plate to be deposited on, otherwise a deposition will be formed on the other side. There are various varnishes fit for this purpose. Melted bees' wax does very well, but I find brown spirit varnish, or shell-lac varnish, to answer best.

If the plates are hung vertically, or the subject deposited on is of considerable thickness, a curious occurrence takes place, often for want of care, but most frequently for want of a sufficient supply of sulphate of copper, more especially when the apparatus is constructed without a shelf, on which crystals of the cupreous salt can be deposited, near the top of the liquid.

In reading the experimental portion of this work, or on referring to the description of fig. 1, page 7, it will be observed, that by placing slips of copper or other metals, in a fluid differing even in specific gravity, that this difference is sufficient to generate voltaic action on two portions of the *same* slip, of which portions one becomes oxidised, and wastes away, while the other is either not wasted, or else it acquires a coating of the metallic solution it may be immersed in,—in point of fact the slip of metal becomes *polarised*.

This phenomenon sometimes occurs in the cells of the apparatus used for copying works of art, but more particularly in those of the sustaining battery, and in all these instances from the same cause; which is, that one portion of the fluid in the cell contains a greater proportion of sulphate of copper than the other. To speak familiarly, the heavier material has *settled* to the bottom. This will always be the case unless the solutions are frequently stirred up, or, which is much better, unless a quantity of the crystalline salt is kept at the top of the liquid, by which means the solution is retained at a uniform strength throughout.

For this simple form of apparatus, I should recommend earthen-ware, if attainable, it being easier cleaned, and where desirous of quickening

the action by additional heat, it is much more manageable than wood or glass, and when made in quantity will be much cheaper than any other material.

Should porous earthenware be used for the medium to separate the fluids, when not in use, they should always be kept filled with water, otherwise the sulphate of copper or zinc that remains in it, when suffered to dry, will crystallise and crack the vessel. If thick brown paper is required as a separating medium, and any difficulty occurs in obtaining it, folds of common brown paper does tolerably well. If care is taken the brown paper will very seldom require renewing.

Next to electro-magnetism there is no branch of science that requires more dexterous manipulation than electro-chemistry; the most trifling film of oxidation often retarding the action of the most powerful apparatus. But, in the present instance, slow action, and simplicity of arrangement being the predominating features, nice attention to minutiae is not so absolutely necessary,—or at least not so much so as to deter those hitherto unacquainted with the subject from practising.

In *all* cases, however, metallic connection is better ensured by binding-screws than by cups of mercury; but, in using them, the copper wire, where the attachment is made, as well as the point of the screw, where it presses on the wire, must be brightened with a piece of emery paper.

In these experiments I have invariably found *equal-sized* plates of copper and zinc to answer best. In the construction of galvanic batteries in general, I do not intend this rule to apply; but my own practice in the present instances has led to the conclusion, that wherever slow and equable action is required, the positive and negative electrodes should be of equal *superficial* area.—Although amalgamated zinc plates are preferable where combined intensity and continuity of action are required, as in the battery, they must not be used, under any circumstances, for the present purposes. It will likewise be found to be essential that the *thickness* of the zinc be equal to that of the required deposition. For if the process is properly performed, the *action* is exactly equal to the *re-action*.

Let the porous bottom of the interior vessel, containing the zinc, be a little larger than either of the electrodes. The zinc should be occasionally taken out of the arrangement, during continuance of the process, and cleansed by washing it in water. The copper obtained by the deposition may be filed, polished, and cut with shears, in the usual manner—the surface acquiring as fine a polish as the copper in use among engravers.

THE

COMPOUND APPARATUS AND ITS MANAGEMENT.

SUBSEQUENT to the preceding processes being made public, while communicating some farther improvements made by myself,* I mentioned some experiments I was at the same time making, with a view to economize, if possible, the electric action generated during the depositing process, by conducting it, by means of wires, into other vessels, where it should deposit copper on the one hand, and generate a sufficient supply of the sulphate on the other.

In the first vessels there were also two pieces of copper, but separated by brown paper, into a pair of cells, in one of which was sulphate of copper in solution, in the other, dilute sulphuric and nitric acid. My object also was, if possible, to collect an increased amount of force, that is observed when the current is transmitted through spiral coils.

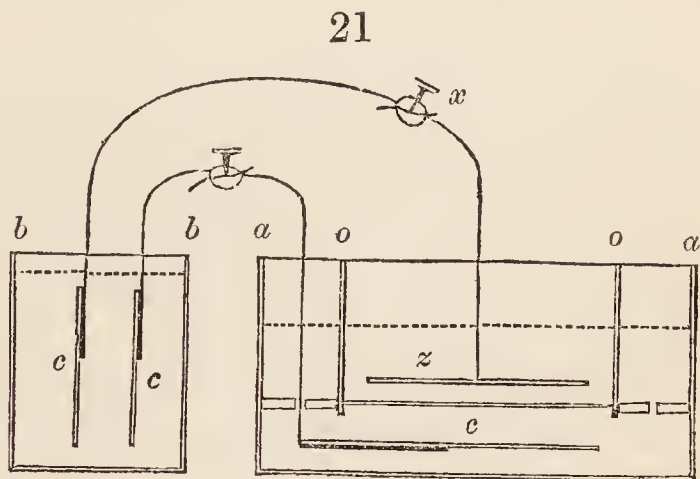
I have been unable to make all the necessary experiments on this latter portion of the subject; but with the means at my command, I have not been able to gain any advantage by the use of the spirals, or if any, so trifling as not to compensate for their trouble or expense. The former portion of these experiments, however, terminated in an economical improvement of the process, more especially should it be requisite to use it on a large scale, and for commercial purposes.

The principles of this improvement are these, and to render them the more intelligible, the reader must refer to the larger figure of the simple apparatus already described. Fig. 20. The electric action is there generated at the surface of the zinc. It is then conveyed through the porous diaphragms to the surface of the copper, or plate to be deposited on; it there performs its function, and is conveyed *along the wire* back again to the zinc. Should we, however, disconnect the wires, and place the ends so disconnected in another cell containing sulphate of copper, we shall find that a deposit takes place on the wire connected with the *zinc*, while oxidation is taking place on the end connected with the *copper*, *just the reverse* of what is taking place in the primary cells.

* See Athenæum, April 18, 1840.

The sum of the useful deposited copper produced by these means, is rather more than what we should acquire by confining the process to the simple pair of cells used in the first instance; but should copper plates be attached to the ends of the wires, and be equal in size to those in the primary cells, we should still have a deposit on one of *them*, and oxidation on the other. The advantages of this arrangement, with a little modification, are, that we have *less waste* deposition, two or more plates may be going on at once, and the voltaic action seems to be kept more equable; and with the additional advantage of simplifying one set of apparatus, by getting rid of a double cell.

This arrangement may be explained by the figure 21; *a, a*, the larger apparatus already referred to, containing a zinc plate in one cell, and a copper or other plate to be deposited on in the other, and separated with a porous diaphragm; *b, b*, another vessel, containing a pair of copper plates, one of which is to be deposited on, while the other will be dissolved, and by such dissolution, afford a continual supply of sulphate of copper, being just an equivalent of that deposited on the opposing plate. This, however, may be increased by adding to the solution contained in vessel *b, b*, a few drops of nitric acid. This promotes oxidation on one of the plates, thereby communicating an increase of electric action to the other.



In my published experiments on this subject, I added a very small portion of sulphuric acid to the nitric, and separated the plate to be copied from the other by brown paper. This arrangement I regarded as the primary cells. In the secondary vessel I put sulphate of copper only, but again separated the one piece of copper from the other by brown paper, to prevent the deposition of partially oxidised pieces of copper on the plate to be copied. This last precaution I thought necessary, should the plates in the secondary vessel be placed horizontal, and the oxidising plate placed above the one to be deposited on, which position, in some cases, I regard as the best; as, when these pieces are suffered to fall on the lower plate, the inoxidised portions of them become deposited on, and are in consequence attached to the plate, and ultimately covered up, depriving the plate of its solidity.

The simplest method of preventing this, is to cover *the plate to be oxidised* with one fold of thin brown paper. This paper must not be suffered to come *into contact* with the other plate, or, should it do so, copper will be deposited *on* the paper instead of the plate. Subsequent experiments have shown, however, that in the *secondary* cells, it is unnecessary to place the plate *to be deposited on* at the bottom of the

vessel, as the fluid remains, as far as I have been able to observe, always saturated throughout, or at least *so long* as there is a portion of the opposing copper plate undissolved. Consequently we may place the plate to be copied *in the secondary* vessel, in any way it may suit our convenience, and perhaps vertically, as in figure 20, may be as convenient, or more so, in some circumstances than any other. It must be borne in mind that should the copper plates, *c, c*, in the secondary cell be placed far apart, the action in *both* vessels will be considerably retarded: in fact, by these means, we possess a convenient method of regulating the deposition. Should we lead wires out of the secondary vessel into a third, with a view of depositing a third, or even a fourth plate, then the action in all will be retarded considerably, and the amount of the deposition in all will only be the sum of the first pair of secondary plates.

I wish it to be distinctly understood, that this has no reference to the arrangements in the primary cells, or where the copper has to be deposited from, already formed sulphate of copper, and excited by the oxidisation of zinc or other oxidisable metal. I can easily imagine a form of apparatus, where they might be so placed if desirable, but not so with those already described.

The manipulatory instructions necessary in this method, differs little or nothing from those already given. . Respecting the secondary cell, it may be necessary to say, that it must be filled with a saturated solution of sulphate of copper, with a few drops of nitric acid, this helps the decomposing process. The plates in this cell should never be so large as those in the primary or exciting cells, also the wires, however joined, whether by soldering or otherwise, should always be so *in the centre* of the back part of the plates. The action by this means radiates equably from the junction point to the circumference. When the wires are fastened to the ends of the plate, it is invariably observed that at the other end a greater amount of action has taken place. This is true with respect to each of the electrodes. In this respect the electrical force acts on the copper, analogous to the magnetic phenomena in iron or steel, the ends or poles always exerting the greatest force.

DEPOSITION WITH THE BATTERY.

SINCE the former processes have been made public, several writers on the subject have recommended the use of the galvanic battery, and the deposition to be carried on in a separate cell. A writer also in the *Philosophical Magazine*, recommends it as the best method of procedure.*

It may be worth while to examine the value of this method, as compared with the others. With myself, an economical mode of producing the deposited copper, or other metal, has always been the main object, and the question has been to find the most simple and least expensive means. By the use of the battery, are these desiderata attainable? I think not.

To make this appear, let us suppose we have a battery of 12 pairs of 4 inch plates, which is by no means a large one, and that we are desirous of copying an engraved plate in copper, and that it is required to be as thick when completed as one of the zinc plates of the battery, and of equal superficies.

Let us now suppose the plate to be deposited on arranged in a separate cell, and that another copper plate shall be opposed to it, to be dissolved, and that these plates are attached to the battery, forming its electrodes. The battery being excited by dilute acid, deposition commences, and a copper plate of the given thickness is formed, but let us see at what expense? First at the expense of the dissolution of the 12 *zinc plates* of the battery, and of the copper plate opposed to it in the secondary cell, the only advantage gained by this process being an abridgment of the time by a little more than one half.

This will be at once apparent when the fact is taken into consideration that every portion of the electricity evolved by the battery is caused by the decomposition of water; or, in other words, for every equivalent of water decomposed, an atom of zinc must be dissolved *in each cell*; hence it will be at once evident that with a battery of a thousand pairs of plates, no greater quantity of electricity will be evolved than that which would be given out by a *single pair of plates*. The only advantage gained being intensity, which, in this case would shorten the time.†

* See *Philosophical Magazine* for June, 1840.

† Faraday researches, and Dr. Thomson on Heat and Electricity.

It has been imagined that the sum of the electricity evolved in all the cells could be collected and brought to bear on a given point, and act *as a whole*, such, however, is not the fact; but, on the contrary, no more deposition can be obtained in any one cell than there is decomposition going on in another, and should there be *one* small cell in the arrangement, *all* the others are *brought down to its level*. In this instance, the vessel containing the plate to be deposited is in reality *one of the cells* of the battery, being connected with it in the same manner as the other cells.

It is possible, however, to arrange the plates of an extensive series as one cell, and that the plates, however numerous, shall act as a single pair; but by doing so, we come back to the system laid down in the first instance, viz., a plate of copper and an *equal* sized plate of zinc; and by adhering to this as much as possible, all the complicated and expensive arrangement of binding screws, and numerous plates is avoided, and, in reality, more deposited metal is obtained, at much less expense.

Wherever large surfaces are required to be deposited on, single pairs, equal to the task, may be much more readily constructed than a similar arrangement of battery plates.

Since the simple forms of apparatus were engraved, described in fig. 17 and 20. I have had another constructed of a different form, which promises to be as useful as any I have yet contrived. It must be borne in mind, my experience of it is limited. I have not had sufficient time to have a sketch of it engraved to illustrate the work; but it will be easily understood, by referring to fig. 23, which represents it as nearly as possible, by supposing the two wires, *y* and *x*, to be joined by a binding screw.

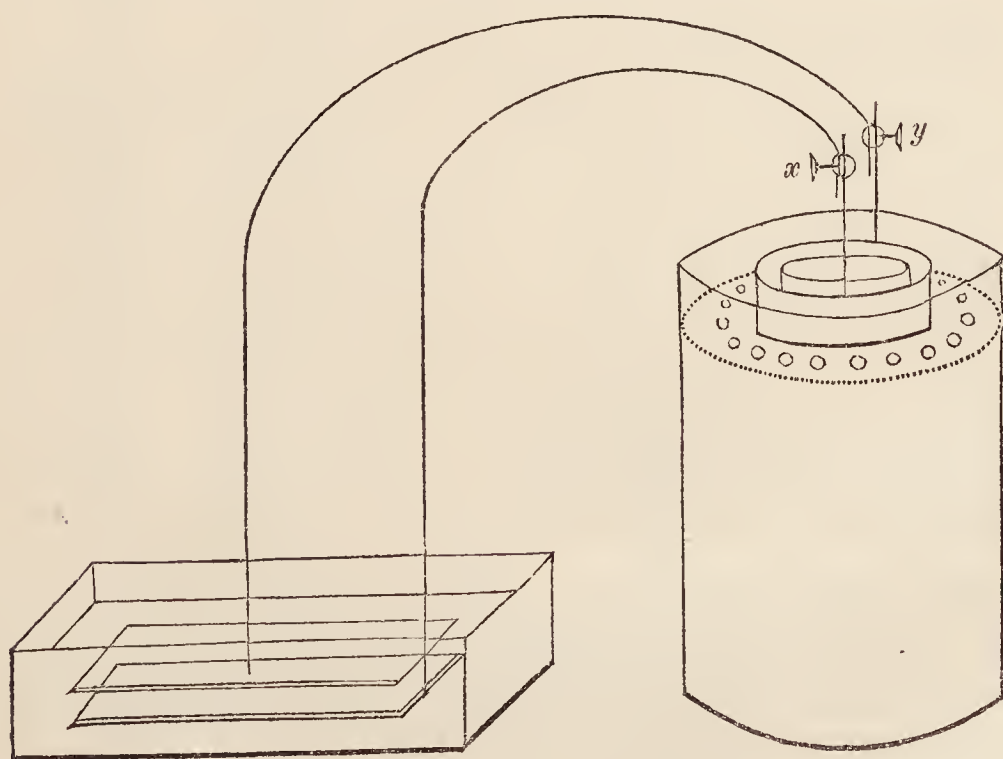
From the apparent convenience of this form of apparatus, I would have adopted it from the beginning, but found that the lower end of the deposited plate, was always much thicker than the upper, the wires attaching the plates being always for convenience sake soldered at the upper end. This difference of thickness was attributable to the different strength acquired by the fluid when at rest, the lower portion being often saturated, while the upper portion was exhausted of its salt of copper. I have now been able to remedy this, by soldering or otherwise fastening the wire in the *centre* of the plate. This apparatus is an earthenware box, of any size, but divided into two cells, by a division of porous earthenware, about $\frac{1}{8}$ th of an inch thick. This may be removable at pleasure, which is sometimes necessary, by causing a groove to be made in the sides of the box, and slipping the plate of unglazed ware into it, and making tight with plaster of paris. The cell into which the plate to be copied is put, should have a projecting shelf near the top, in the manner of a pneumatic trough, and perforated with holes, on which should rest undissolved crystals of the sulphate of copper. The zinc plate must be placed in the opposite cell, excited as usual.

BATTERY PROCESS.

WHERE plates are to be copied in haste, a voltaic arrangement of a single pair may sometimes be advantageous; but *the negative plate* of the apparatus might just as well be a plate to be copied as not, as the electricity generated at the positive plate, is capable of performing that office *without any diminution* of its power, as developed in the secondary cell. Where time is not an object, it is just so much electricity wasted; but should a circumstance occur, where speedy execution is of much greater consequence than economy, the subjoined rules and description of apparatus will be found useful.

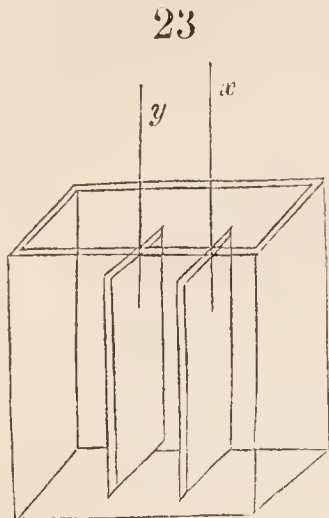
Fig. 22 represents a battery arrangement of a single pair—the electrodes of which are a pair of copper plates, placed in an adjoining cell,

22



one of them being the plate to be copied. These may either be placed horizontally as in the cut, or vertically, as in fig. 23, being connected by the wires *x y* to the adjoining battery pair.

The battery pair is of that species usually termed sustaining, for the first idea of which, I have reason to believe, we are indebted to Professor Daniell. A single pair of this construction may be made of any requisite size, from a pint gallipot and upwards, their size to be regulated by the size of the plate they are required to copy.

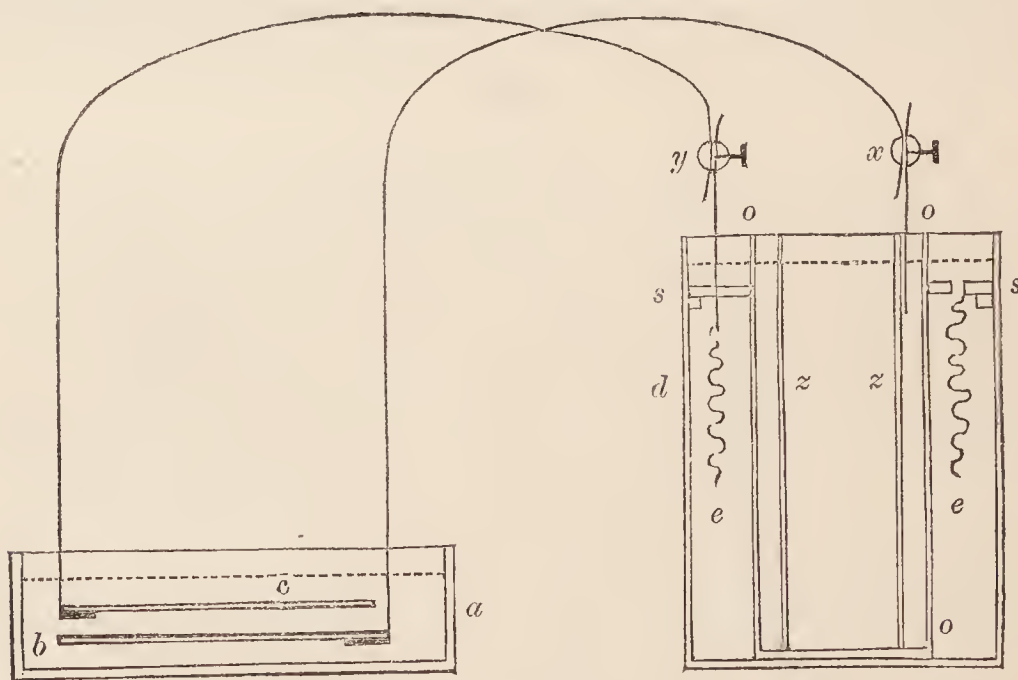


The plate to be copied, should never have *more* superficies than the copper or zinc plate of the electro-motive pair. It is better that they should have less; as, for instance, should it be required to copy a plate having 20 inches of superficies on the side to be acted on, then let the superficies of the zinc in the primary arrangement have from 30 to 40 inches, and the action will be much

better kept up, and copper formed more rapidly; but should there be very great disparity of size between the plates in the primary cell and those in the other, the action will be either too slow or too quick. The first, when the plates forming the electrodes are much larger than those in the primary cell, and the latter, should they be very much smaller.

The annexed, figure 24, is a section of figure 22, which I shall here describe minutely. *d* may represent a gallipot, being 8 inches high,

24



and 6 inches internal diameter. This is a most useful size. The undulated line *e, e*, is a cylinder of copper, $6\frac{1}{2}$ inches high, and 5 in diameter. *o, o*, represents another cylinder, but made of some porous material. Where quick action, as in experiments with the battery, is required, brown paper cylinders are preferable, but for the action requisite to deposit copper, I find that unglazed gallipots are very convenient. The porous vessel, when in its place, must be of the same height as the exterior gallipot, and 4 inches external diameter. This may be made of exactly

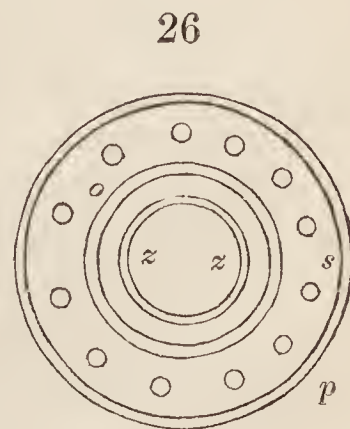
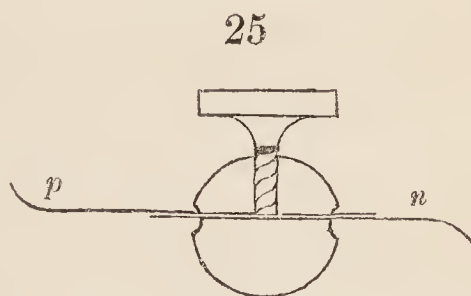
the same material as the common gallipots, but merely finished without being glazed. The thinner they are the better. $z z$ a cylinder of zinc, which is to be placed in the interior of the porous vessel. This must be of as great diameter as possible, in order to bring its surface near to that of the copper, this being an important object in all battery arrangements; still the external surface of the zinc must not quite touch the surface of the porous vessel, but be about one-eighth of an inch distant from it. $s s$ is a perforated collar of earthenware, which fits into the interior of the larger gallipot, and may either rest on the copper cylinder, or on a small rim made in the vessel. This, when in its place, will be about an inch or more from the top of the outer gallipot, and is for the purpose of containing a quantity of undissolved crystals of sulphate of copper, to supply the waste by the deposition, and to facilitate this, it is perforated with holes. This contrivance I find very useful, as the fluid in the external cell is always kept in the state of saturation. To each of the metallic cylinders, wires are to be soldered, and these connected with the wires of the plates in the adjoining vessel by the binding screw, a section of which is given, fig. 25, $n p$ being the wires.

Pieces of apparatus differing from this are usually soldered to the plates, or cylinders of battery arrangements. Being somewhat inconvenient and expensive, this one is proposed as an improvement on the former kind, and at once abridging the required number in battery arrangements by *one-half*, being moveable in any required direction, and fastening *two*

wires at once. Binding screws of the former description were a vast improvement over mercury cups, but I anticipate that the electrician will find those proposed a still farther improvement; a quantity of them should always be in his possession, to enable him the more readily to perform any experiment that may suggest itself. Their shape is of little consequence—they may be square or oblong, but I imagine the proposed round shape, with two sides flattened, will be found best. The square vessel a represents the secondary cell. b . the plate to be copied, and attached to the *zinc* cylinder of the battery. c the plate to be oxidised and attached to the *copper* cylinder of the battery.

Figure 26, a plan of the battery, and showing the perforated shelf. p the external gallipot; $z z$ the internal cylinder of zinc; the other lines represent the porous cylinder and the copper in their relative positions, with the perforated shelf to contain the undissolved crystals. This bat-

tery must be excited by sulphate of copper and sulphate of zinc, a saturated solution of the first, and a weak solution of the second, the



action of the battery instantly causing it to acquire strength, until it becomes saturated.

The interior of the zinc cylinder should be varnished, as this prevents action from taking place on it. The copper and zinc cylinders, in this modification of the battery, should not be soldered together at the edges, but be merely bent into a cylindrical shape. Should the copper cylinder be plain, it is advisable to have it perforated near the top, with holes to allow of a free transmission of the fluids from one side to the other. The external dimensions I have laid down, are only given as convenient for ordinary purposes; they may be of any size, all being regulated by the outside gallipot, but the dimensions of the other portions of the battery must bear *the same relation to each other* as those given. Care should also be taken, that all the cylinders are concentric with each other. Should the action require acceleration, an increase of temperature, by heating the solutions, is by much the best means of acquiring it. All I have stated with respect to the primary pair of plates, may be applied to the galvanic battery for ordinary purposes, the most extensive being only a series of pairs of plates, placed in connection, either by soldering the connecting wires, mercury cups, or binding screws; and the rules laid down are applicable to both. Care being taken that throughout the whole series *copper* and *zinc* are connected alternately.

THE GALVANIC BATTERY.

FOR performing galvanic experiments generally, I have preferred what is usually termed the sustaining battery; and, notwithstanding all I have recently seen in scientific periodicals, respecting the powers of those lately introduced of another character, the adapters of each must pardon me, if I am somewhat sceptical of their capability to *continue* and *sustain* the same amount of action for an indefinite period; indeed I have never seen any species of galvanic battery, but the sustaining one, that was capable of retaining its power undiminished for half an hour after the commencement. At *half* the lapse of that period, an unequivocal diminution takes place.

Their principal claims, however, on the attention of the scientific has been the great amount of power developed a few seconds after they were in action, this being exemplified by the decomposition of water, and the amount of its constituent gases collected in a given period; but even in this latter particular, they are excelled by the sustaining apparatus, if properly constructed. In this respect, I speak from my own experience only, but do not wish it to be implicitly relied on, without the concurrent testimony of others; and that opportunity may be afforded for this, I shall describe the species of battery I use, for what may be termed lecture table experiments. It is a modification of the

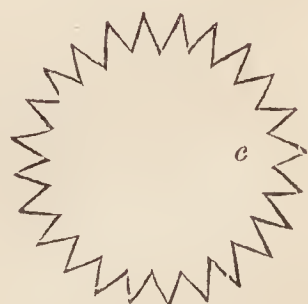
one proposed by Professor Daniell; but the objection I found with this gentleman's, as originally described, was the smallness of the surface of zinc (it being a rod) as compared with the copper, and the consequent *distance* of the surface of these metals from each other. The membranous substance, proposed for the porous divisions, have always been found objectionable, they being seldom able to perform twice without decomposition, and consequent leakage. Besides, it is exceedingly inconvenient to renew them, and their manipulation is disagreeable.

It has been found, that the copper surface of plates or cylinders used for the battery, should be much larger than the zinc, but no statical experiments, that I am aware of, have yet been made to show the relative sizes of each, or why they should be so. In the absence of such, I have imagined that the extent of surface might have some connection with the gases evolved, as we know, that at the zinc or positive side of the battery, we have an evolution of oxygen, which, when collected, occupies a given space, and that at the copper or negative side, we have an evolution of hydrogen, which invariably occupies *double* the space of the gas collected at the other side.

Reasoning from these facts, although I must confess vaguely, I was led to construct cylinders of copper, having exactly *double* the superficies of the zinc. In this, however, another difficulty presented itself, in the distance that intervenes between cylinders, having such disparity in their diameters. When a zinc cylinder was constructed, having 24 inches of exposed superficies, a copper one of double the amount, or 48 inches, if placed concentric with it, would necessarily involve a distance of an inch between the opposing surfaces of the metals. This, experience has abundantly proven, is greatly too much. In fact, the *least* distance compatible with the intervention of the porous division and a portion of each fluid will, I apprehend, be found best. With proper arrangement, three-eighths of an inch is amply sufficient, and I am convinced that at that distance the maximum power *is attained*. In order that I might obtain *double* the amount of surface, and yet not exceed this distance between the metals employed, I resorted to a very simple expedient. I procured some very thin sheet copper, and crimped it, as in the subjoined fig. 27, being a horizontal section of a cylinder so formed. By referring to the figure, it will be at once obvious that by adopting this form I obtained *double* the superficies of the zinc, and yet its distance never exceeded three eighths of an inch from *any portion* of the copper. The greatest power of this battery has been obtained by the adoption of brown paper cylinders for the separating medium. The mention of these is generally associated with fragility, and lack of durability, but this is altogether a mistake.

When properly formed, they last longer than any porous substance I ever experimented with, and are quite as manageable. They may be made in

27



the following manner:—Procure a turned block of wood, precisely the diameter of the proposed brown paper cylinder, which should be *an exact medium* between the two metallic cylinders, also a thin piece of wood turned of equal diameter, but having a slight groove in its edge, in the form of a wheel, this forming the bottom of the cylinder. Now, fold a piece of stout brown paper round the cylindrical block, having previously cut the paper to the required dimensions. The piece of wood with the groove in it, will now fit into the end of the folded paper, and may be secured in its place by binding it tightly with pack thread, the thread fitting into the groove. Should the paper not be very stout, and the battery required to be used with hot solutions, a double fold or more of the paper will be necessary, but do not apply cement of any sort, as it would melt with the application of heat; but where this is not intended, and where thick paper can be procured, I prefer only one fold, overlapping a little at the junctions, and then fastening it with cement, just as a piece of tin is soldered, the cement answering the purpose of solder. The top edge of the cylinder may be folded over toward the outside, and this folding also secured by cement, thus forming a rim to secure the top. Should the cement cool before the joint is considered good, as it is likely to do, run a moderately hot iron over the surface, before removing it from the wooden block, and this will secure it. I have cylinders so formed that have been in use two years, and are not leaky. When the paper is of the proper thickness, they retain their cylindrical form, and will stand perfectly upright when filled with a fluid. When a battery thus formed is not required to be in action, it is unnecessary to remove these, or the copper cylinders from the solutions, the zinc only sustaining injury, by remaining immersed. The exciting fluids for this battery, are saturated solutions of sulphate of copper in the outside cell, and sulphate of zinc or of soda in the interior one. Wires are of course soldered to each cylinder, with as *little* solder as possible, and any number connected together by the binding screw.

As economy has ever been an object with me in the construction of apparatus, I have adopted it wherever it was compatible with efficiency, and, to render a battery still more so, I have occasionally used very thin lead, instead of copper, it being much easier crimped into the required form, and *equally* efficient. Whenever the lead is immersed in the cupreous solution, and in connection with the zinc, it becomes *covered with solid copper*, and in a few minutes plays the part of a copper cylinder, which it subsequently continues to do. A battery thus constructed, with a shelf or circular collar, already described, to contain undissolved crystals of the cupreous salt, may be kept *in undiminished action* for weeks, and at a very slight expense. Any of the single arrangements so constructed, will be found most economical for the depositing processes already described.

I shall add a few words respecting the powers of this battery, and in doing so, it will in some degree be necessary to compare it with others. That proposed by Mr. Grove, is very powerful in a small com-

pass, its power being estimated by the decomposition of water, and the evolution of gas. This method of testing the battery, I am not prepared to say is the best possible, but as yet, experiments are wanting on the subject. The negative series are platinum, and the cells are separated by porous vessels. It is, altogether, a good arrangement. The next recent one is by Mr. Smee. He has recently deposited, by the voltaic process, platinum upon silver, and the plates thus prepared are associated with amalgamated zinc. This battery is by no means so powerful as that of Mr. Grove, and is, on the whole, much more expensive. Besides, it has several disadvantages. It must be borne in mind, that the platinum is deposited on the silver plate, in the state of black powder, which adheres to the surface but slightly, in consequence of which, the silver below becomes occasionally exposed. Whenever this is the case, an action takes place the *very reverse* of that intended. The platinum and silver, being in different states of electricity with respect to each other, a *local current* is at once established, which *retards* the primary action; but should silver plates *alone* be taken, having *no* deposit of platinum on their surface, *the action is much more powerful*. I have used arrangements of silver and amalgamated zinc, with dilute sulphuric acid, and although I have not made strict observation, it would certainly appear that a battery on this plan is quite as powerful as any hitherto known. In fact, there is no limit to the combinations of metals that may be made to form a voltaic arrangement, Sir H. Davy having pointed them all out a quarter of a century since, including even charcoal; but the real object should be to find the best, or that which combines the desiderata of economy and *continued* power, it being small encouragement to the experimentalist to find, that while arranging a portion of his apparatus, the power of his battery is reduced to zero, and that to regain it, a fresh charge of acid is required, while the zinc plates are considerably reduced in thickness.

Mr. Sturgeon has lately adopted a species of battery, which most certainly has the merit of being not only simple, but cheap. It may be very briefly described, by stating, that it consists of a series of cast iron *gallipots*, the interior of which not only serves to contain the dilute acid, intended to excite the battery, but also acts as *the negative series* of the arrangement. In other words, instead of using copper and zinc, this gentleman uses cast iron and zinc; and, from what I have seen, it is as effective as any of the others lately produced. This battery acts best when *covered* with *rust*. Otherwise, in fact it is very feeble.

I cannot help thinking Mr. Sturgeon has not paid sufficient attention to this latter circumstance. When investigated thoroughly, it may lead to some important results respecting our views on this subject.

May not the circumstance have some connection with facilitating the attraction of the hydrogen to form water, the supply of oxygen being acquired from the oxide already formed on its surface?

I have now briefly mentioned the best of the recent improvements in

the battery; but, with deference to the authors of each, I most unequivocally prefer the sustaining one. If not quite so powerful in the beginning, still it is quite as much, or indeed more so, at the end of a few hours than at the commencement. I occasionally use a small arrangement of six gallipots, containing little better than a pint each. It consists of six plates of crimped *lead*, made from that used to line tea chests, and as many brown paper cylinders of one fold, with an equal number of cylinders of amalgamated zinc. These are excited by sulphate of soda, (glauber salt,) or sulphate of zinc in one cell, and sulphate of copper in the other. When this is in action for a few minutes, the lead becomes covered with copper. The *exposed* surface of each of the so formed copper cylinders is 42 inches, each of those of zinc being 21 inches. With this I am able to collect three cubic inches of the mixed gases *per minute*, and this action may be kept for days if required. Should the temperature of the solutions be increased to 120° or 150°, nearly *double* this amount of the gases may be collected. Indeed, I have never seen any voltaic arrangement, that at all periods of its action will exceed this, with an equal amount of exposed surface. The data here given is rather under than overrated. It will be obvious, that where increased surface is an object in the negative series, this method of obtaining it may be adopted to increase that of any form of battery.

